THE

VETERINARY JOURNAL

OCTOBER 1901.

Editorial.

THE ECONOMICS OF VETERINARY TEACHING.

UNDER this heading our readers will remember we published an article in our April number. We are glad to see it has attracted the attention, amongst others, of the editor of the *American Veterinary Review*, who devotes an editorial to the subject in his August number. The writer says: "Our worthy colleague gives as a support for his claims figures which seem so ridiculous that it can scarcely be understood how a staff of good worthy teachers can be expected." He then goes on to say that it is true things are not much better in the United States, are in some cases worse, and he asks why this is so. "It is," he answers, "because so far, and with very few exceptions, veterinary teachings have been mostly private undertakings, and only in the two or three institutions which were endowed were special fees paid to teachers; but if rumour be true, these last schools were obliged in some way or other to change their mode of operation,—yes, suspend work." He adds the startling sentence, "Veterinary teaching as a private undertaking has no business to exist." But what is then to become of veterinary students in Great Britain and in America? In America, according to the *Review*, endowed institutions seem either to have become defunct or are decaying, and only the efforts of praiseworthy private individuals maintain a feeble life in the veterinary schools of the country. In Great Britain eliminate the private teacher and where is veterinary teaching, and veterinary science too? Would there exist those long rolls of famous men who have done brave work for their king and country on many a weary march, in many a tedious campaign? Would disease have been fought with and crushed under foot? Would our food supplies be so wholesome or so abundant, our markets so busy, our merchants and our farmers so prosperous? We fancy that the editor of the *Review* does not mean exactly what his words indicate. He is like Oliver Goldsmith, who was once questioned by an unsympathetic stranger as to the meaning of the first line of the "Traveller," and answered that the words were there to speak for themselves; they were ordinary words used in the ordinary way.

XUM

This aroused the wrath of Dr. Johnson, who happened to be present at the interview. He told Goldsmith that he was quite mistaken, that the words, simple as they were, bore a deep significance, a sublime meaning, which the Doctor then and there elaborated, to the great delight of Goldsmith. Now what our American friend means is, we fancy, that the private individual ought not to be asked to bear the burden of a great teaching institution unaided. It is quite enough for the teacher to attend to the teaching of his subject, he ought not to be harassed with considerations of pounds, shillings, and pence. The very word economics has an ominous and doleful sound to the scientific ear. It indicates the painful limitations of human activity, and reminds us that we are no longer in the Garden of Eden. It is like the rain and the wind beating on our window-pane in winter,—a useful reminder, if you like, that unpleasant conditions exist outside, contrasting with the warmth we enjoy indoors, but a paralysing influence when the window gives way and the rain deluges our floor, and the wind scatters our books and papers in wild confusion. No, we believe with the *Review* that scientific teaching and financial questions are best kept apart, each having its own sacred place and its own high priests. Let there be a friendly, a close alliance, but not a combination. Of course this can be accomplished only when a wise Government takes the matter in hand, with an Education Department and a Treasury Department both at its disposal; or it can be accomplished by the extension of the scheme of a university which shall see the wisdom and importance of affiliating veterinary to medical teaching. This, as our readers well know, is an act of justice—we might say evolutionary justice and wisdom—which we have persistently advocated, and which we hope to see brought within the sphere of practical politics before long.

The editor of the *Review* points to a danger that seems to threaten. He speaks of the possible multiplication of Colleges in America and elsewhere, and the consequent flooding of the market. In case this should seem somewhat inconsistent with what has been already said, we will act the part of Dr. Johnson once more, and amplify our friend's meaning. If neither Government nor University aid be forthcoming, the honourable and conscientious private teacher will probably succumb, almost necessarily, as has been said, but will a mere void be left? We think not. As the decay of true religion, the theologians tell us, brings about all kinds of horrible superstition and idolatry, so will the decay of true scientific teaching open up a wide field for charlatanism. Where formerly enthusiasts existed quacks will abound, and the last state of the country shall be infinitely worse than the first. For the quack has a thick skin. He has no soul, and therefore no future. He makes the rustics stare, and they are happy for the moment, but they come back to sober reality by and by; they look about them for the quack, but he has gone, and so has their money. Let

not the Royal College of Veterinary Surgeons flatter itself with hopes that may prove delusive. It alone can confer veterinary degrees, it is true, but if the supply of carefully taught veterinary surgeons be artificially restricted the public may accept cheap imitations with an imposing appearance, and perhaps a bogus degree obtained in America. These are painful reflections, and we fear our editorial will read like a jeremiad, but after all it is no use to cry peace, peace, when there is no peace.

Original Papers.

SOME OF THE DEPARTMENTS OF THE REMOUNT COMMISSION AND THEIR WORK.[1]

BY PRINCIPAL W. O. WILLIAMS.

SOME of his hearers, he said, might not be aware of the enormous charge which the Veterinary Department had in South Africa. Statements of the numbers had, however, appeared recently in the papers, viz.:—

197,000 horses,
50,000 mules,
200,000 oxen, and
500,000 sheep,

or a total of 947,000 head of stock being looked after by the so-called Army Veterinary Department, assisted by the civil veterinary surgeons. He did not think that altogether there were 200 veterinary surgeons looking over that, roughly speaking, 1,000,000 head of stock, and yet the Army and Civil Department had come in for a considerable amount of blame for the great losses that had occurred in South Africa. One could quite understand that with the Cape, Natal, Orange Free State, and the Transvaal, or nearly half of Europe, 200 veterinary surgeons had a most difficult and enormous task. Orders had been given that any animal which could not be cured in six weeks should be destroyed. The result was that if an animal had a sore back which could not be cured in the stipulated time, it was simply shot. Not very long ago 500 were shot at Pretoria. This, therefore, accounted for the enormous losses. Were the veterinary surgeons afforded an opportunity and the horses a chance, the losses would not be so severe as at present. It would, no doubt, have been observed that within recent years the sphere of usefulness of the veterinarian had very much enlarged, and his field of work greatly expanded. Large demands were now made upon the veterinary profession, not only by our municipal authorities, military authorities, and State Animals Department, but also by the Governments of our Dominions and of our Colonies. In these circumstances, it behoved veterinarians to keep in the van of veterinary knowledge and things pertaining thereto, because he felt sure that one of the new and permanent fields opening up to the profession was that of remount work. For these reasons he, as a non-military man, ventured to

[1] Abstract of a paper read before the Quarterly Meeting of the Lancashire Veterinary Medical Association on September 5th.

describe some of the departments of remounting. The remounting organisation in this country was not to be compared with that of Continental countries, where for many years one system or another had been in good working order. Here there had never been what might be called an efficient remount department, but since the war broke out, endeavour had been made to improve that department. In foreign countries, were an order given for 20,000 head of cavalry, not only could 20,000 trained men be rapidly turned out, but also 20,000 trained horses. Here, at the beginning of the war, when a demand was made for so many thousand cavalry, probably 20,000 efficient men were called up, but only 10,000 horses could be found, and the country had to be scoured to get anything ungulate to carry the men. This would never occur again, for the War Office was being reorganised, and one of its branches, the Remount Department, would be sure to be efficiently staffed so as to meet the future large and regular demand and any emergency call. This large annual demand ought to be easily maintained in the country without a single importation from abroad, and, in his opinion, in time of war excessive quantities of stock should not be taken out of the country, but the excess obtained from the colonies or from foreign countries. Accepting the view that this country would have to buy abroad, it would be necessary to obtain a thorough knowledge of the following :—

(1) The horses of the country : their characters, values, and special diseases ;
(2) The dealers in that country ;
(3) The markets ;
(4) The food-stuffs ;
(5) The accommodations ; and
(6) The means of transportation and exportation.

All these things ought to be known by the Veterinary Department, and, generally speaking, no class of men were so capable as they of knowing or learning about those things. There was no reason why in time of peace bunches of horses should not be purchased from foreign countries so as to test our staff to see if their methods of working were efficient. In all probability this will be done in future. In this country there were foreigners who seemed to know almost every good horse in it, its whereabouts and value, and if they bought it they knew the safest and most economical way of exporting it. In foreign countries we had men just as knowledgable, but they worked for themselves or large companies, and not for Governments. England, like France, Germany, and Japan, ought to have a remount staff in every country where she purposed buying, and the remount staff should remain there and become acquainted with the items he had mentioned. He understood that in the United States of America there were now, and had been for some years, efficient remount staffs of foreign countries who were thoroughly conversant with the knowledge he had indicated, and he would not be surprised that if England went to war with one of those nations, and gave orders in a hurry, as was done at the outbreak of the Boer War, she would find probably that not a single horse could be bought at a reasonable price, seeing that the foreign remount staffs would have "options," thus preventing England purchasing anything at all,

After these preliminary remarks, Principal Williams then detailed the procedure in connection with the purchase and shipment of, say, 1000

horses or mules from America to South Africa—a procedure now being carried on in America. The contract for those was divided amongst a certain number of responsible dealers, who deposited sums of money or produced guarantees that they would fulfil their promise if they got contracts. The contract would probably be divided among five dealers, each of whom would have to produce 200 animals at a certain date. On the appointed date arrangements would be made for the purchasing officer and his veterinary surgeon to visit a certain stockyard. The animals are brought out singly before the purchasing officer, who glances at them, and if he approves of their general appearance and stamp, the veterinary surgeon then examines as to age and height. Age and height being correct, the veterinary surgeon roughly examines them as to soundness.

These tests being passed, the horses are branded on the fore feet, and then become British remounts. After being branded, they are tested with mallein, and if correct, are usually sent from the dealers' pens to grazings in some part of the country (say) in Missouri, or in Texas. Out of the 250 which the dealer might bring forward, probably the purchasing officer might get his 200 made up. If not, the dealer would have to bring more horses next day until the complement was made up. It was not an uncommon thing for the purchasing officer and veterinary surgeon to examine and purchase 150 horses in a day. That was, however, a very hard day's work. As a rule horses and mules were purchased for certain ships,—for example, 1500 for the "Milwaukee," 1000 for the "Iona," &c., and when the particular ship for which the animals are intended is notified as about due in New Orleans, usually ten days prior to her sailing, her stock is called for. The thousand head of horses were placed in cars, called horse cars, and these were the worst types of car there were. Being boarded in closely at the top, the animals could not get any fresh air, and the lower portion being open railings, the wind blew round their legs, in consequence of which a considerable number contracted colds and chills, and often arrived at New Orleans with pneumonia or pleurisy. The cars, which held twenty-six horses each, and very often from twenty-eight to thirty mules, had in them feed water troughs,—also hayracks in them; but as the animals stood some with their heads to one side, and others with their heads to the other side of the car, as frequently as not the horses' fæces were placed in amongst the food in the manger, and consequently the other horses would not touch the food. The horses having to travel a considerable distance from their grazings, usually it was necessary for them to have a rest on the way, and coming, say, from St. Louis or Kansas City, they would go through Memphis, and probably stay there. They would be taken out of the cars and put into sheds or stockyard, watered, and fed, and allowed to have a few hours' rest, then reloaded. The unfortunate part of the loading and unloading was that the niggers had a love of long sticks, with nails driven in, and in pushing the horses into the cars it was not an infrequent thing to shove the nails into the animals, causing abrasions, and in several cases perforations into the sinuses of the head. Immediately the animals arrive they were usually sent down in special express trains of from 200 to 250, and on arrival at New Orleans they were placed in pens or yards, which were always kept clean and ready for them. In New Orleans surprise is expressed at this country's freedom from great outbreaks of disease. Cleanliness was not well known out there. His regular practice was to give the animals hyposulphite of soda in their drinking water to

XUM

ward off "acclimation" fever, which was really a bad form of influenza, contracted by the animals in filthy places. While the thousand horses or mules rest at New Orleans they are daily inspected by veterinary surgeons, and all the sick ones picked out. There was a great deal of tetanus in New Orleans, but he used to give the injured animals antitetanin, with the result that he never had a case of tetanus. While resting, a gang of blacksmiths went through the whole of the stock, and in the case of the mules took off all the shoes and shortened the toes. Mules which were worked on farms or ranches, or on cotton or grain places, had feet 2 or 3 inches too long. With the horses their hind feet shoes were taken off, their toes rasped down, and their fore-feet shoes removed. In the winter time when they came from the north they were usually very heavily clad, consequently a gang of clippers clipped them. This clipping prevented the animals from getting attacks of pneumonia. The ship being ready, and the thousand head of stock also ready to be loaded, there were two loading-places in New Orleans,—one called Southport, and the other Slaughter-Houses. It was therefore only necessary to drive the stock from the yards into the shoots, and put them on board. If to be shipped from Southport, they were loaded into trains at the pens in the town, and taken by car up to the railway wharf, across the latter, and on to the ship. According to the size of the vessel it was determined whether the loading was to be from two, three, or four alleyways or shoots. The shore-mouths of the shoots were on the edge of the railway wharf, and were the same distance apart as the middle doors of two railway cars, so that when a train pulled up with the door of the first car opposite the mouth of No. 1 shoot, the doors of the next three cars would be opposite their respective shoots. For instance, four cars were unloaded simultaneously. The first horse was led out and the others followed. This was not done with the mules, because the first usually made a rush and a man would be trampled under—moreover the mules had not got their head-stalls on. Mules required to have shoots much higher than those for horses. If horses, the first one had a halter-shank clipt into its head-stall and was then led to its place, and as each horse followed on board it had its halter-shank clipt on and followed its leader. It was really marvellous the intelligence they displayed in keeping their heads down when necessary, and in picking their way over any ships' standings. A car holding twenty-six was emptied by a gang of thirteen niggers in two lots. Of some thousands of horses and mules loaded in his presence in New Orleans, he had only one serious accident, which occurred to a horse going down a shoot from one deck to another deck, and when half-way it took fright and tried to jump out on the deck, but split its skull on the coamings of the hatch and dropped down dead. After this rough idea of the passage of horses from the time of purchase until taken on ship, he asked his hearers to follow him on board. The first thing to be noticed was that all the stalls were made extremely secure, of strong wood with uprights between them, which were 4 by 4. The breast-boards (one to every five horses), usually 12 inches in depth and from 2½ to 3 inches in thickness, were bolted on to the beams, so that there was no danger of the horses breaking down their breast-board when the ship rolled. Vessels without bilge-keels rolled enormously, but those with them did not roll; consequently the animals had greater comfort. The stalls on the upper deck being strongly braced together there was no chance of breaking down in a rough sea. On board ship most of the places were

fitted for the conveyance of horses or mules, but the animals ought not to be placed near the engines. Another thing to be carefully watched was that the stays from the funnels did not get between a horse or mule's hind legs. He knew of horses which had stood the whole of a voyage with a long stay between their hind legs. The fittings ought to be of the correct measurement. The stalls were usually 6 feet 6 inches long; some, however, were 8 feet, but he approved of 6 feet 6 inches as sufficient. This gave a horse a good distance to swing, but not sufficient to pitch off its feet. The width of the stall should be 26 inches, which was a decent width for a horse. The floor ought always to be of timber, and on the floor should be cross-bars to keep the animals from slipping. Some cattle boats have asphalte floors, which are extremely dangerous. Then the feed-troughs differed. At New Orleans the troughs were divided into partitions, so that each horse or mule had a separate trough of its own, in which its feed was placed, and the hay could be fed by placing it in nets hung at the standard, or from the troughs. Here large zinc buckets which can be fastened with hooks on to the breast-board are sent out with the feed. Water is carried in buckets, which are filled from hydrants throughout the ship. The allowance of water per day was 8 gallons, so that a vessel going on a month's voyage (twenty-eight days) would require to have a ton of water for each animal. On walking round a vessel it would be found that 5 per cent. of the stalls were left empty, and these were for the sick animals. The poop and fore part of the deck should be reserved for sick animals. In America an allowance of five men to 100 animals was made, but as some of the men would fall sick there would probably be only three men to the 100, so that it could easily be understood that these men had very hard work. Here an allowance of one man to every two or three horses was made. Animals on the upper decks and in the holds did well—in fact one of the best parts was right down in the lower hold, whereas the worst part was that called "'tween decks," which was seldom more than 7 or 8 feet high. It was the largest deck on a vessel, and as several hundreds of animals might stand all the way round in "'tween decks," it was extremely difficult to ventilate that part of the ship, consequently there would be more sickness there than in any other part of the vessel. The best possible method of ventilating was by having uptake and downtake shafts worked with steam fans. One of the most difficult parts of the vessel to ventilate was immediately behind the bulkheads, and animals were very liable to be suffocated there unless ventilated properly.

MENINGITIS AND ITS ALLIED AFFECTIONS.[1]

BY E. WALLIS HOARE, F.R.C.V.S.

MR. PRESIDENT AND GENTLEMEN—I have selected as the subject for discussion this evening, the clinical history, pathology, and treatment of meningitis, and its allied affections.

Now, technically speaking, the term meningitis would only apply to an inflammation of the meninges or envelopes of the brain and spinal cord. But clinically we cannot differentiate between affections of the meninges

[1] Read at a meeting of the Irish Central Veterinary Association on June 25th.

and of the brain itself, of an inflammatory character, so that it will be necessary to widen the subject.

At the outset we must admit our ignorance on many points in connection with diseases of the nervous system. A reference to current veterinary literature will show what we have yet to learn on the subject, and a little consideration will explain the reason why these diseases are left comparatively unexplored.

In the first place they are usually not amenable to treatment, or they are fatal in their results.

Again, in consequence of the situation of the organs involved, there is great difficulty in exposing them for post-mortem examination; very careful manipulation is necessary, and a skilled pathologist is often requisite in order to detect lesions in these regions. Indeed, we have often been disappointed, after going to the trouble of examining the brain and spinal cord, to find no marked lesion present, although the symptoms presented by the case were suggestive of an interesting and instructive autopsy.

Although the diseases we propose to consider cannot be classed as of every-day occurrence, they are of sufficient importance to merit a discussion as to causation, prevention, and treatment. A comparison of experiences is the best method of elucidating facts in connection with diseases of an obscure nature, and often gives more useful and valuable information than scientific researches in the laboratory.

It is not my intention to bring forward anything new or startling on the subject, but simply to group together a few clinical observations, and then to depend upon your superior judgment for explanations, instruction, and criticism.

We may take it for granted that when acute inflammation of the meninges occurs, the brain itself will be involved to a greater or less extent. No doubt text-books give us certain diagnostic symptoms on the question of cerebritis and meningitis, and some practitioners of superior attainments in this direction may fancy that they can differentiate the various nervous affections, but clinical experience, which is the best teacher and the most reliable guide, leads us to believe that when the symptoms which we shall shortly describe are present, we cannot go beyond a diagnosis of inflammation of the brain and its membranes.

Let us glance for a moment at the anatomy of the parts involved. We have the brain surrounded by three envelopes.

Externally the dura mater, a fibrous membrane closely attached to the inner surface of the cranial bones and forming their internal periosteum. Its outer surface is connected to the bones by a number of fine fibrous processes and vessels, while the meningeal vessels ramify on the same surface. The inner surface is smooth and covered by an endothelial layer, it is intimately applied to the brain, which is contained within the next two membranes. Processes are detached which pass inwards, forming partial partitions between different parts of the brain. The dura mater forms certain sinuses through which important blood-vessels pass.

The next membrane is termed the arachnoid, which is like a serous membrane, its parietal layer being formed by the endothelial layer of the dura mater, the visceral layer surrounds the brain and covers the pia mater. Between the parietal and visceral layers is a space termed the *Arachnoid or subdural space*, the free surface of the membrane bounding this is smooth and moist. Between the visceral layer and the pia mater is another space

termed the *Subarachnoid.* This space is continuous with the same space in the spinal cord and contains the cerebro-spinal fluid.

The pia mater is the internal covering of the brain. It is closely applied to the latter, and its structure is composed of delicate areolar tissue and blood-vessels, so that it has been termed the vascular membrane of the brain.

From this brief description it will be seen how delicate, vascular, and highly organised these structures are, and how serious must be the effects of acute inflammation therein. Also in consequence of the continuity of the dura mater of the brain and spinal cord, there is a tendency for the inflammatory process to spread from one structure to the other.

As already remarked, the post-mortem appearances, in cases that presented violent symptoms during life, are often not well marked, and we are at a loss to understand how a congested condition of the meningeal and cerebral vessels, with effusion into the subarachnoid space and ventricles, can produce so formidable an array of symptoms.

In some text-books a variety of affections are included under the heading of "Inflammation of the Brain and its Membranes." Thus we have phrenitis, stomach staggers, grass staggers, sleepy staggers, &c. It will be as well to try and make a distinction between these conditions.

"Staggers" might be applied to a variety of affections in which a staggering gait is a prominent symptom. We must confess that we know very little concerning the real nature of what is known as "stomach staggers." Some authorities attempt to explain it as depending on engorgement of the stomach, which acts in a reflex manner on the cerebrum. We see cases presenting symptoms suggestive of engorgement of the stomach, and in addition a sleepy appearance and a staggering gait. Many of these recover after the action of a full cathartic dose. But in others, symptoms indicative of acute inflammation of the brain and its membranes present themselves, and a fatal termination is the result. In others again the autopsy does not show an engorged stomach, but a condition of acute gastritis is present.

These affections, however, should be carefully distinguished from what is known as "Megrims," which affects the horse suddenly while at work; and, when the convulsive fit has passed off, the animal appears perfectly well.

Under the heading of *Enzootic Paraplegia*, Robertson describes an affection occurring as the result of feeding largely on rye grass. It is also known as grass staggers. He regards it as a spinal affection, gradual in its development, with a low mortality, and depending on some specific toxic influence exerted by rye grass at some particular stage of its growth, and under certain conditions. Not having met with any of these cases I shall not refer further to the subject.

The cases to which I wish to direct your special attention are those in which no deleterious agent exists in the food, and no appreciable cause can be assigned. We do not often see these cases at the commencement of the attack. As a rule we find the patients lying down, unable to rise, and in violent convulsions. In some instances the first symptoms presented are a staggering gait, a disinclination to move, a sleepy appearance, with a tendency to force the head against the manger or wall.

In a variable period excitement occurs, and the animal may walk round his box, the body becomes stiff, there may be a twitching of the superficial

XUM

muscles, and the *membrana nictitans* may be drawn over the eyes, as in a case of tetanus. There is marked constitutional disturbance present, and after a short time the animal usually goes down, struggles in a violent manner with fore and hind limbs, and sweats profusely. There are short periods of quietude, but without any warning the convulsions recur with great violence. The eyes are moved from side to side in a quick convulsive manner, and the *membrana nictitans* is drawn over the eye and suddenly retracted. The pupils are dilated in the secondary stages, and as a rule the temperature ranges very high, in some instances being 107° or 108°. The head is constantly forced backwards while the animal is lying down: this is a very constant symptom. The animal may get up again and wander about the stall, knocking his head against surrounding objects. More commonly, however, he is unable to rise, and the convulsive movements of fore and hind limbs continue, making it dangerous to approach him. At intervals he may take a bite of hay in his mouth, or even feed from a bucket, but eventually he becomes comatose and dies.

The autopsy in every case that I examined showed acute gastritis, in some a large amount of ingesta was present in the stomach, in others comparatively little. The brain and its meninges showed the appearances described as indicative of acute phrenitis, but in very different degrees. In some there was a large amount of turbid fluid in the subarachnoid space, giving the brain an opaque appearance. In others the most striking appearance was the congestion of the pia mater, the most minute blood-vessels being injected.

In one year I met with a large number of these cases, and, whether by a coincidence or otherwise, they appeared about the same time. While doing temporary duty at a cavalry barracks, I saw two cases attacked within a few days. A posting horse close by was affected in a similar manner a few days after; in this animal abdominal pain preceded the cerebral symptoms.

When we are called to these cases and find them unable to rise, it is often difficult to arrive at a diagnosis. In the latter stages of azoturia we have precisely similar symptoms, probably due to some toxic agent, the nature of which we are not aware of, acting on the brain and meninges. The urine at this stage may be clear in appearance, and we have only the history of the case to assist us. In chronic renal affections, when uræmia occurs, convulsions of a similar character appear, and an examination of the urine is necessary to form a correct diagnosis.

We now come to an important point, viz., the *cause* of meningitis in the cases we have mentioned. Various causes are suggested, but, leaving out traumatic cases, we must admit that we are as yet in the dark with reference to this subject. To term such idiopathic is no doubt convenient, but is of no assistance to the question of prevention.

We cannot as yet explain in a satisfactory manner why it is that a horse may get a severe attack of abdominal pain and next day be attacked by acute laminitis. We may try and get out of the difficulty by advancing the theory of metastatis, or of sympathetic nervous influence, or, what is more fashionable in the present day, ascribe it to the influence of germs. Well, it is rational to suppose that meningitis may occur in a similar unknown manner, and a certain proportion of cases of acute abdominal pain are followed by laminitis, and a smaller proportion by meningitis.

One would imagine from the situation of the central nervous system that it would be less liable to attacks of acute inflammation from the

source we have mentioned than is the case. We are almost inclined to believe in the sympathetic nervous influence unless some more satisfactory reason is advanced.

We have met with cases of meningitis as a complication of the pneumonia of influenza. The patient may appear to be progressing favourably after a severe attack, when he will suddenly go down and exhibit the characteristic convulsive movements. In these instances we might justly believe in a bacillary origin of the affection.

Severe exhaustion is mentioned as one of the causes of meningitis, and I have met with a few instances in posting horses where the symptoms appeared a short time after the animals arriving from a long journey. The autopsy showed acute gastritis in every case.

Meningitis may occur from traumatic causes, such as fractures of the carnial bones, in which sharp spiculæ may penetrate the membranes. In cases of compound fractures it is probable that septic influences contribute to the result as well as the direct injury. A case came under my care lately in which a brewery horse got "wedged" in a narrow stall at night and showed evidences of prolonged struggling next day. The breathing was stertorous and the power of deglutition was lost, but at intervals the characteristic convulsive struggling occurred, and I ordered the animal to be destroyed. No autopsy was possible, as the case was twenty miles from town.

Referring again to the question of "Stomach Staggers" it would be interesting to inquire as to the real pathological condition of the cerebrum in the cases that recover. The sleepy appearance of the animal, the tendency to press the head against surrounding objects, and the staggering gait suggest some alteration in the cerebral circulation, the exact nature of which we are unable to explain. But when such cases do not terminate favourably, it is my experience that violent symptoms always supervene, indeed it is very seldom that the horse, no matter what disease he may be suffering from, departs from this life without a final struggle.

We shall now briefly refer to meningitis as affecting the spinal cord. Here again it is difficult if not impossible to differentiate affections of the membranes from diseases of the cord itself. Nor is it always possible to definitely determine whether the cerebrum and its membranes are not also involved in the affection. The causes are often obscure, those given by authorities being external violence, and exposure to cold and wet when fatigued.

I have clinical notes of one case, a harness horse that had been sent a long journey by boat in cold weather. The first symptoms observed were a straggling gait in both hind limbs, and marked hyperæsthesia along the spine and gluteal muscles; indeed the animal would resent the slightest touch of the hand. The symptoms increased rapidly, the animal constantly lifting each hind limb from the ground in a convulsive manner, and apparently without any control over these movements. He went down the same evening, struggled in a convulsive manner, and died early next morning in violent convulsions. The autopsy showed the meninges of the spinal cord thickened and inflamed. The horse had been recently "docked," and the stump was in a very bad state. The owner stated that when the animal came off the boat he noticed a peculiar action in both hind legs. Whether the "docking" had anything to do with the spinal affection or not is a matter for discussion.

XUM

Cerebro-spinal fever or epizootic cerebro-spinal meningitis I have never met with, nor am I aware of its existence in this district.

Time will not permit me to notice meningitis as it affects cattle, and as I have no experience of it in these animals, I hope you will fill up the blank by giving your experience on the subject.

In the Dog meningitis may arise from traumatic causes from disease of the internal structures of the ear, and from violence inflicted on the head. It may accompany or be a sequel of distemper, and in such instances the symptoms presented may simulate those of rabies.

In some outbreaks of distemper we find meningitis of rather frequent occurrence, especially in highly bred dogs, and it is evidently due to some action of the distemper virus on the meninges. Some of these cases are characterised by regular convulsive fits, which finally terminate in a comatose condition. In others these fits may be absent, but the animal becomes very irritable and bites on being handled, gets into secluded corners, the eyes become inflamed, and the sight may be lost, paralysis gradually supervenes, and the animal dies in a comatose condition.

Chronic Meningitis *may* exist in the horse, but I am not aware of any diagnostic symptoms in connection with it.

In the dog it is of more frequent occurrence. The following will illustrate some of the phases that occur in cases of this nature. This case has a melancholy interest for me as it occurred in a valuable bull-terrier dog that I imported from Manchester. The age was eleven months, and he had only arrived a few weeks when I noticed that he occasionally staggered when observing any object. When running in the field he would sometimes appear to lose control of his movements, and later on would fall over if there was any obstruction in his way. He also seemed deficient in locating sounds, and if called would go in the wrong direction. These symptoms became aggravated, and he would fall completely over when running. As he was constantly eating large quantities of grass I came to the conclusion that it was a case of stomach derangement, and after a purgative dose he appeared perfectly well. He showed a continual tendency to force his head against any one that came near him. The next symptoms that presented themselves were a tendency to go to the right side when walking, the head was turned to the left side, the nose pointing slightly upwards, there was loss of control over the right side of the body, if the head was slightly elevated the animal would champ the jaws, and fall over on the right side. When feeding the animal would snatch up each hind leg in a convulsive manner. The appetite was voracious, and the animal's condition very fat.

For about three months the symptoms continued in greater or less degree, some days there would be marked improvement, and a course of iodide of potassium seemed to give beneficial results. Finally, the symptoms became more pronounced, the hind limbs became paralysed, convulsions of a violent character supervened, and I destroyed him with chloroform.

The autopsy showed acute inflammation of the meninges, on the left side the dura mater was adherent to the other membranes and very much thickened. The brain itself was extremely congested, and this organ when covered by the meninges was of a dark bluish-red colour, more marked on the left side. The stomach shows evidences of chronic gastritis.

The points of special interest in this case are the gradual development,

XUM

no appreciable cause, the intermittent character of the symptoms, and the periods of apparent improvement.

Treatment.

We now come to the question of the treatment of the affections we have been considering. We must admit that when meningitis has developed and the patient down and struggling, treatment of any kind is of no avail, and the most humane procedure is to destroy the animal.

Every treatment has been tried, bleeding, sedatives, purgatives, anodynes, chloral, and even inhalation of chloroform, but all with the same result. Our attention should then be directed to the early treatment of those affections that are likely to terminate in meningitis.

Assuming that in many instances certain gastric derangements may terminate in cerebral complications, the first indication is to administer an active cathartic, aloes and calomel in full doses being the best agents. It is of the utmost importance to clear out the alimentary canal, and to divert the flow of blood from the brain.

Bleeding is recommended by most authorities in the early stages, and whether the symptoms be of commencing violence or sleepy in character, it is indicated. It is of course useless afterwards. If the patient be excitable, nerve sedatives are indicated, of these chloral hydrate and bromide of potassium are reliable agents.

Local applications I do not consider of any value, and if there be any chance of success it must be brought about by prompt and active internal treatment.

Of course, as I have previously remarked, we do not usually see these cases in the early stages, and consequently are able to do little, if anything, for the patients when the disease is fully developed. I think it will be generally admitted that affections such as meningitis, stomach staggers, &c., are not prevalent in the present day as compared with former times. More attention to feeding, better regulation of work, and less injurious fatigue, are no doubt accountable for this.

In conclusion, I must apologise for the indifferent manner in which this paper is put together, but the short time at my disposal, and the fact that it has been written in moments snatched from the routine of a general practice, will be the excuses that I beg to offer. If it should have the effect of exciting an interesting and useful discussion, and thus improve our clinical knowledge of the subjects noticed, I shall feel that my humble efforts hrve been amply rewarded.

SURGICAL TREATMENT OF EXUDATIVE PERICARDITIS IN THE OX.

Two Clinical Lectures by M. G. Moussu, Alfort Veterinary School.

Gentlemen—This morning I bring to your notice a patient suffering from exudative pericarditis. All the symptoms are the clavical ones, and all are present except the murmur and splashing of the *liquor pericardii*. We have seen many such cases, and I have explained to you the various com-

XUM

plications that may exist, according to the degree of distention of the pericardium, the consistency of the fluid, and whether there are adhesions between the heart and pericardium, the pulmonary pleura and pericardium, &c.

The animal before you moves with difficulty and a staggering gait, and the attitude assumed is diagnostic; the respirations are laboured, the head and neck extended, and the fore limbs straddled widely apart from each other.

What first catches the eye is the enormous œdema of the dewlap of the lower part of the neck and the jugular channel, the enormous extension of their veins, and the regurgitation present, with the congested condition of the mammary veins. Precussion reveals an enormous pericardial effusion that is as cognisable on the right as the left side. The sounds of the heart cannot be distinguished on auscultation, and after moving a few yards they become completely disorganised.

The respiratory can be heard round the edges of the distended pericardium, but so faintly as to be almost inaudible. The pulse at the maxillary artery is nearly imperceptible.

All these symptoms prove that there is pericarditis with great effusion, and this diagnosis was arrived at several weeks ago by my friend M. Parent, of Malesherbes.

The prognosis, as you are aware, is generally considered to be fatal, for although cases of simple exudative pericarditis are met with that are cured either by medicinal agents or spontaneously, the majority are caused by foreign bodies that cannot be located, and the only course that can be taken is slaughter. To this course there are objections. The flesh of such carcases is soft, infiltrated, and watery, not only the fore but the hind quarters. It is not in the strict sense of the word diseased, but it is poor, inferior meat, and in towns would be condemned. No doubt, in country places, where inspection is lax, it might be sold, but generally such carcases go to the knacker, not the butcher.

If, therefore, the œdema can be by any means reduced, there is no reason why such meat should not be sold, as it is not inflammatory, and this is the sole object of the experiments that I have made and the means I have devised, as the matter appears to me to be one of practical economy.

The œdema and infiltration of the tissues is caused by the difficulty the blood has in returning, due to pressure on the heart, particularly on the auricles. If, therefore, the pressure can be relieved, the stain will be removed, the œdema absorbed, and the meat can be sold for food. That is what constitutes the problem.

In theory nothing could be more simple. It would appear that all that was necessary was to evacuate the fluid by puncturing or incising the pericardium, but up to the present a practical method of doing this has not been devised.

I must before going on bring to your notice a few facts in topographical anatomy.

In the ox the heart and pericardium are situated in the plane of the median line of the body, the point coming almost to the insertion of the diaphragm with the xiphoid cartilage. The two pleural sacs descend nearly to the sternum, and on account of the cardiac tube of the left lung being smaller and thinner than the corresponding one of the right, it is not in contact with the walls of the thorax.

From experience gathered from many post-mortems I have found that even in cases of exudative pericarditis with a large amount of effusion, it is very rare to find an adhesion between the thorax, lung, and pericardium; and clinical observations have shown me that it is impossible to diagnose it even if it does exist, and for practical purposes we may take it for granted that the two pleural sacs remain permiable to the sternum. The importance of this point will be seen later on when we discuss the operation. In the case before us the respiratory sound at the lower border of the lungs is clear though feeble, and this alone allows us to say that if the pleura and cardiac tubes of the lungs are inflamed by contiguity to the diseased structures, which is usually the case, they nevertheless have not ceased to act. They are not consolidated, neither are they altogether adherent to the pericardium on one side or the thoracic walls on the other.

In order to evacuate the pericardial fluid it is necessary to have recourse to puncture or incision.

Puncture is easily performed but incision is a delicate operation, and is inadvisable unless one is certain that the portion of lung between the pericardium and the wall of the chest is adherent on *both* sides. If it is not, or the incision is made outside this adhesion, pneumo-thorax is certain to supervene, which, in the case of a patient whose heart only acts with difficulty, is almost certain to be fatal. I have tried both these operations but have abandoned them.

Puncture of the Pericardium.—This operation is simple enough, especially with Potain's aspirator. The extent of the pericardial effusion should be first determined, and then the site of the puncture (between the fourth and fifth rib) should be cleaned and disinfected. The canula is inserted and the fluid flows into the receiver.—(Fig. 1.)

Nevertheless there are difficulties, of which I will now speak. If the canula is of small calibre it is invariably obstructed, as the fluid always contains flocculi floating about in it. If a large canula is used the results are most unsatisfactory for the following reasons: The fluid is always septic and generally putrid. The pericardium is inflamed, thickened, and becomes inelastic. When the canula is withdrawn the puncture wound remains patent because the tissue does not contract, and a small quantity of this highly septic fluid escapes into the pericardial sac, and after a few hours septic pleurisy sets in, which carries off the patient in a very short space of time.

This is the reason why puncture, which would appear at first sight to be *the* operation for the relief of cardiac pressure in cases of exudative pericarditis, has such bad results and why I have termed it a failure.

If there is double pulmonary adhesion between the pericardium and thoracic wall, then no doubt this procedure is perfectly safe, but as it is impossible to diagnose such adhesion it may be dismissed from practical surgery.

Pericardial Incision.—Having had such bad results from puncture, I next had recourse to incision through the wall of the thorax, but with no better success.

I chose the fifth intercostal space for the opening, taking care not to wound the internal thoracic artery or vein. It is necessary to operate somewhat high up and cut deep. This is not so easy to perform as it at first sight appears to us.

I have also resected the cartilages of the fifth, sixth, and seventh ribs

XUM

and incised the pleura, but this procedure cannot enter into the field of practical surgery.

Trephining the Sternum.—Remembering the situation of the lower point of the pericardium, and in order to avoid interfering with the lung and pleura, and to prevent the occurrence of pneumo-thorax, I trephined the sternum on the median line just in front of the xiphoid cartilage.

I hoped by this route to get directly and easily to the pericardium, and to make the puncture in a most favourable portion at the most depending part. I had, however, not considered that the great œdema would make it difficult to clear the bone sufficiently to allow the trephine to work, and the danger of throwing an animal suffering from pericarditis. This danger is real, the patient expiring from syncope as soon as thrown, or during the struggles they make, and I learned the fact that it is dangerous to throw an animal affected with pericarditis.

Puncture of the Pericardium in the region of the Xiphoid.—Having found the operations described unsuccessful, I devised the following. The apex of the pericardium lies on the sternum, almost at the insertion of the diaphragm, and the sac is always in contact with the xiphoid at the neck where it joins the sternum, being only separated by a small layer of fat. If you will look at the diagram (Fig. 2) you will see that the distended pericardium is in close proximity to the xiphoid.

Theoretically it would seem possible to gain access to the pericardium on either the right or left side between the neck of the xiphoid and margin of the ribs, and traverse the fatty cushion at its apex.

If in practice this turns out to be feasible, "and I see no reason why it should not be," it will have the great advantage of opening the pericardium without wounding any serous cavity and without touching the peritoneum or pulmonary pleura, a great advantage, as all immediate complications are avoided.

The space in which the operation can be performed is, no doubt, restricted, but is sufficiently large to allow the posterior mediastinum being reached at the apex of the percardium. The course of the puncture should be slightly oblique from behind forwards, and without inwards, so as to the fatty cushion being traversed, and the pericardium pierced without risk to the pleura. The point for the puncture is easily located, the neck of the xiphoid serving for a guide. This operation appears to me to be practicable and free from any great danger, also it can be performed on the animal standing.

I intend to perform it a little to the left of the median line on a level with the neck of the xiphoid. I shall make an incision through the skin and the anterior insertion of the abdominal and posterior of the pectoral muscles, so as to come direct on the neck of the xiphoid, which I shall free from all tissues in the angle formed between it and the ribs.

I shall then with my right forefinger feel for the pericardium and break down the fatty cushion, and then, having located it, will puncture the sac. The results of this experiment I will communicate to you on a future occasion.—(*April* 24, 1901.)

Lecture No. II.

Gentlemen—It is just fourteen days ago that I explained my views of the possibility of surgical intervention in cases of exudative pericarditis and sketched out the proposals of a new operation. This has been per-

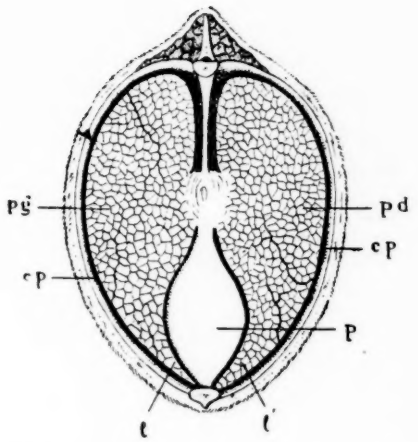

FIG. I.—Diagram of section through the thorax at the level of the heart.

Pg. Left lung.
Pd. Right lung.
cp. Pleural cavities right and left.
P. Pericardium.
ll'. Cardiac lobes interposed between the pericardium and thorax walls.

XUM

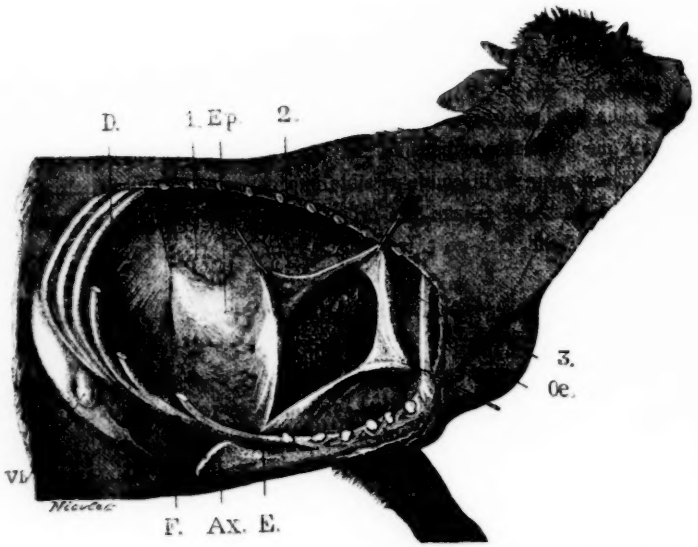

FIG. II.—Exudative pericarditis covered by a foreign body. Showing relationship of the pericardium with the sternum and xiphoid pericardial sore opened.

D. Diaphragm.
OE. Œdema of the dewlap.
Ax. Xiphoid.
F. Liver.
Vb. Bile bladder.
1. Lung.
2. Cardiac lobe.
3. Anterior lobe.
E. Site of puncture.

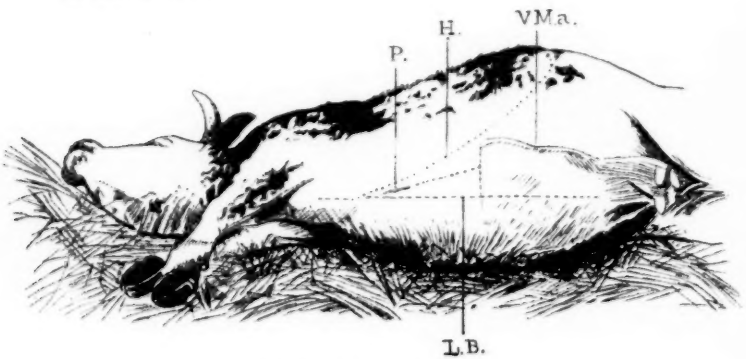

FIG. III.—Site of operation.

L.B. Linnea alba.
H. Margin of ribs.
V.M. Milk vein.
P. Site of puncture.

formed without cutting the animal, and I am now about to explain the results.

Our patient now walks and breathes easily, and the enormous œdema of the dewlap has altogether disappeared. The appetite has returned and is great, and although the sounds of the heart are irregular and badly deliminated, there is nevertheless great improvement, and the case is much better than I had any hopes of.

When I proposed this operation all I had in view was to relieve the cardiac pressure and promote absorption of the œdema, so as to render the meat saleable. This has all taken place, our patient has taken a new lease of life, and there has been no complication. I intend to keep the subject under observation.

What I wish to explain is the operation itself, which is not a laboratory one, but can be carried out by every practitioner, and this, it seems to me, is its chief value.

I have already explained to you the reasons that induced me to plan it out. We will now proceed to the technique. The patient is left standing, the head and hind limbs being secured.

Three landmarks are then located,—the line of the ribs, "Circle de l' Hypochondre," the linnea alba, and if a cow the anterior border of the mammæ at the point of junction with the abdomen. These three landmarks will form a triangle, and this is bisected at its apex, and at this point the incision is made.

All these marks can be clearly located when the patient is standing, and the incision easily made on the left side. There will be considerable œdema, which must be allowed to drain away, and with the knife the pectoral muscles cleared away from the xiphoid cartilage. With the right fore-finger the fatty cushion is broken down, and if the pericardium is very much extended you will find it, and in any case it can be located some distance away by the impulse given from the heart. The sensation conveyed to the finger is very well marked, and such of you who assisted me can bear me out in this statement from their personal observation. The right forefinger is then replaced by the left, and a trocar 10 inches long and at least one-quarter inch in diameter is introduced guided by the finger till in contact with the pericardium, which can be told by the sensation of fluctuation of the fluid caused by the contraction of the heart being communicated to the trocar and thence to the hand.

The trocar should have a direction slightly oblique from without inwards and forwards towards the median plane to avoid injuring the left pleural sac. The finger is withdrawn, and with a smart tap on the handle of the trocar the pericardium is punctured. The trocar must not be driven more than 1 inch to 1½ inches at the most. The trocar is withdrawn, the canula left *in situ*, and the fluid escapes.

In this case we removed about 6 pints of a non-purulent white fluid, which gave an abundant deposit on standing. It is important not to shift the position of the canula during the flow of the fluid, for if it passes in too high up a considerable quantity may be left behind in the *cul-de-sac* of the pericardium. I washed the sac out with a 9 per cent. salt solution, using about 1½ pints, at a temperature of 104° F., but the whole did not flow back. This, however, did not seem to be any disadvantage. Drainage of the wound was provided for with an iodoform slip of gauze, and a draining applied to prevent contamination from the bedding.

XUM

From the operation manipulations and the escape of the fluid at the side of the canula the wound became contaminated, but it would be impossible to get it aseptic, and having a good dependent opening it was not of much importance. The draining was renewed in forty-eight hours and afterwards every third or fourth day, and at the present time the wound is cicatrizing well.

The œdema of the fore-quarters had almost altogether disappeared after forty-eight hours, and on the sixth day after there was no trace left. The result looked for had been attained, and the beast could have been slaughtered on that day. The appetite, which had been lost, reappeared the day after the operation, and has remained excellent. I must impress you that this has been a favourable case, and you must not run away with the idea from what you have seen that you can effect a permanent cure of all. It is, no doubt, an advance in surgery, but the relief is only temporary, and of course this is the case where this condition of things is due to a foreign body. I fully expect that in this patient there will be a relapse, and it is as a control animal I am keeping him alive. Even in the case of the effusion not being caused by a foreign body, or, if it was, its becoming encysted, a complete cure could not be expected, because with such intense inflammation as is revealed from the character of the fluid adhesions between the heart and pericardium must take place.

That, gentlemen, is all I have to say, but I think that this operation is of some practical use, and will be adopted in general practice.—*8th May* 1901.

Note.—The animal was kept till July without any relapse or recurrence of external symptoms, and to all appearances was in perfect health. It was slaughtered, and on post-mortem examination adhesion of the heart and pericardium were discovered.

(*From Recueil de Médécine Vétérinaire*, 15th August 1901.)

JOSHUA A. NUNN,
Vet. Lieut.-Colonel,
Army Vety. Dpt.

ON FANCY AND IMAGINATION IN THERAPEUTICS.

BY E. WALLIS HOARE, F.R.C.V.S.

DIFFERENT varieties of practitioners are met with. First, those of the old school, who do not believe in new remedies, but depend implicitly on the rules of treatment that they have practised all their lives. They are usually dogmatic in their ideas, and are not desirous to discuss the *modus operandi* of their treatment. It is quite sufficient for their purpose to know that such a medicinal agent acted satisfactorily on several previous occasions, or that such a procedure proved successful in their hands.

They judge exclusively by statistics and results, ignoring altogether the various phases and types that diseases may assume, the idiosyncrasies of different animals, and the recuperative powers of Nature.

To doubt for a moment that their line of treatment did not cure the patient would be regarded as rank heterodoxy.

In consultations, they freely admit that unless their dicta are carried out, the patient is not getting a chance for his life.

They regard with scorn any one who is sufficiently bold in his opinion as to doubt the magical effect of drugs, and to ascribe many of the recoveries to the assistance of Nature.

According to their ideas, something must be done in every case, whether it be the pouring down of drugs, or the active measures of counter-irritation, or rubbing the ears and legs.

Some activity must be displayed in order to spur on the flagging powers of Nature, and both attendants and the veterinary surgeon must be doing something in this direction.

To think of a possible error in diagnosis would be to admit want of skill, while the idea of holding post-mortem examinations would be in their eyes puerile, and only suited to the junior practitioner, who, like the young bear, "has all his troubles before him."

And, as practitioners of this dogmatic turn of mind advance in years, they become still more conscious of their infallibility, until they are prepared to say on every occasion—

> "I am Sir Oracle,
> When I ope my mouth
> Let no dog bark."

Curious indeed are some of the antiquated drugs and agents that are employed by the type of practitioner we are describing.

Fever balls and fever draughts and condition powders are dispensed with a lavish hand, while the time-honoured preparations of antimony are still regarded with the highest therapeutical respect.

The kidneys are the organs that most frequently receive the benefit of their attentions, and it is wonderful how these excretory organs are able to withstand the pressure that is brought to bear on them by the administration of hosts of diuretics.

As regards success in treatment, we hear often enough of the number of "cures" these egotists have effected, but nothing of their failures.

If a patient dies, it was not the disease killed him, but natural weakness of his constitution; otherwise, under their skilful treatment, he must have recovered.

In surgical therapeutics practitioners of this variety have similar notions.

The hot iron still holds the field for the arrest of hæmorrhage, caustics and escharotics are still preferred to the free use of the knife; the caustic clam is used for the operation of castration, while blisters and setons are preferred in the removal of tumours to the modern surgical methods. Antiseptics are sneered at as being the ideas of theorists, while in many instances surgical instruments are kept in a condition suggestive of being a breeding-ground for germs. Indeed we have heard it remarked on more occasions than one that a jack-knife and a twitch were all the instruments necessary.

The other type of practitioner to which we intend to refer is the opposite to the one we have described. We say type, because fortunately both are exceptions to the majority of practitioners of the present day, but they are in sufficient numbers to merit consideration.

NOVA ET VETERA.

Telegony.

Knowledge upon the subject of telegony until recently depended entirely upon recorded cases, and although physiologists in past ages have accepted it more or less generally, and created theories to account for it, it has only within the last few decades aroused scientific interest.

Although with breeders generally belief in "infection" has long been an axiom, some, such as Settegast and Kuhn in Germany, and Sir Everett Millais and others in England, have never known a case to occur, and although Millais believed in telegony as an abnormal and rare occurrence, he expressed himself upon the subject thus: "In a breeding experience of nearly 30 years' standing, during which I have made all sorts of experiments with pure-bred dams and wild sires, and returned them afterwards to pure sires of their own breed, I have never seen a case of telegony, nor has my breeding stock suffered. I may further adduce the fact that I have made over 50 experiments for Professor Romanes to induce a case of telegony in a variety of animals—dogs, ducks, hens, pigeons, &c.—and I have hopelessly failed, as has every single experimenter who has tried to produce the phenomenon."[1]

Various biologists have expressed opinions on the subject. Agassiz believed that the male influenced the ovaries of the female. Darwin, after dealing with the question of "the direct action of the male element upon the mother form," sums up with the words: "We have . . . seen that in the case of plants the male element may affect in a direct manner the tissues of the mother, and with animals may lead to the modification of her future progeny."[2]

Romanes considered the evidence which he had collected "enough to prove the fact of a previous sire asserting his influence on a subsequent progeny."[3] He, however, came to the conclusion that it was an extremely rare occurrence, probably not taking place in more than 1 or 2 per cent. of cases.[4] Weismann does not believe in telegony, although he does not dispute its possibility, and he gives an explanation of how it might occur, which I shall refer to later.

Professor Cossar Ewart, who has for many years been carrying on breeding experiments to investigate, among other important questions, that of telegony, says:—"Since I turned my attention specially to the subject, in 1894, I have, in addition to making experiments likely to give telegony the best possible chance of declaring itself, investigated a large number of alleged cases of 'infection.' The result, so far, is that the evidence in support of undoubted 'infection' having ever occurred is most unsatisfactory. In every case investigated the supposed 'infection' could be accounted for by the relatively simple reversion hypothesis, and the same hypothesis, I am now satisfied, is sufficient to account for what at first appeared as evidence of telegony in my own experiments. I do not by any means say telegony is impossible, that it never has occurred in the past, and never will occur in the future; but I think I am justified in saying 'infec-

[1] *Two Problems of Reproduction*, 1895, p. 20.
[2] *Animals and Plants under Domestication*, i. pp. 437, 438.
[3] *Contemporary Review*, April 1893.
[4] *An Examination of Weismannism*, 1893, Appendix ii.

tion' has never been experimentally produced, and that the kind of 'infection' so widely believed in by breeders, if not impossible, is at least extremely improbable—as improbable as the almost equally common belief that the colour of the offspring may be influenced by 'mental impressions,' as Laban's sheep and cattle are said to have been influenced by the peeled wands placed before them by his son-in-law, Jacob."[1]

Now the "tradition," as Weismann calls it, in which most breeders believe is that an "infected" animal "throws back" to his previous sire—that the subsequent offspring take after the first sire. These are expressions commonly used, but they convey no definite idea. Professor Ewart says that breeders believe in direct "infection," and the following example which he gives will indicate what he means by this: "My sable collie's pups to a Dalmatian have only four or five patches of colour, such as occur in foxhounds and pointers. According to the breeders' view of telegony, her next pups to a collie like herself should, if she has been 'infected,' be spotted like a Dalmatian."[2] He therefore evidently means by "direct infection" that the subsequent offspring will resemble the previous sire and not the offspring of the previous sire.

It is rather unfortunate, therefore, that he should have used the term "direct infection" in another sense in the following passage: "If telegony is due to the direct 'infection' of the germ-plasm of the dam by the germ-plasm of the first or of a previous sire, there seems no escape from the conclusion that the subsequent progeny should resemble the first offspring rather than the previous sire."[3]

Now in order to illustrate that breeders believe in the direct infection first referred to, Professor Ewart gives the following two cases, which, he says, makes this evident. "The racehorse Blair-Athol had a very characteristic blaze. . . . It is said that mares, after having foals to Blair-Athol, produced Blair-Athol-like foals to other sires utterly unlike Blair-Athol. In the same way a fox terrier, after having pups to a Dalmatian, had . . . spotted pups to a dog of her own breed."[4] . . . With great respect to Professor Ewart, in the absence of information as to the offspring to the first sire in each case, these examples, as stated, do not make his contention evident. Had he not drawn a distinction, by the illustration of the collie pups, between resemblance to a previous sire and resemblance to his offspring, the cases might have stood, but in view of that distinction they seem to be merely somewhat loose statements of cases of infection, and not to bind the person who states them to a belief in direct infection such as Professor Ewart suggests. I have indeed been unable to turn up anything making it clear that the general belief is such as Professor Ewart suggests, and for the most part breeders, in talking over the subject, do not seem very certain what "infection" does mean when the question is pressed home.

A few cases and opinions will make it clear that the idea of the resemblance of telegonous offspring to the previous offspring has long been current. "Mr. Giles put a sow of Lord Western's black and white Essex breed to a wild boar of a deep chestnut colour; the 'pigs produced partook in appearance of both boar and sow, but in some the chestnut colour of the boar strongly prevailed.' After the boar had long been dead, the sow was put to a boar of her own black and white breed—a kind which is well known to breed very true, and never to show any chestnut colour—yet

[1] *Penycuik Experiments*, p. lxviii.
[2] *Ibid.* p. lxix.
[3] *Ibid.* p. cxxxix.
[4] *Ibid.* p. lxviii.

from this union the sow produced some young pigs which were plainly marked with the same chestnut tint as in the first litter." [1]

Darwin mentions two other cases in which the subsequent offspring resembled the offspring of the previous sire, and also states that "farmers in S. Brazil . . . and at the Cape of Good Hope . . . are convinced that mares which have once borne mules, when subsequently put to horses, are extremely liable to produce colts striped like a mule."

Weismann [2] mentions the following case: "The philosopher Carneri mentions a case which came under his notice. He kept a herd of cattle of the dark grey Mürzthal breed. On one occasion he put one of the cows to a 'light-coloured Pinzgau' bull, instead of to one of the same breed. The cow threw a calf with the characteristic brown and white patches of the Pinzgau breed, as well as with distinct traces of the 'dark grey Mürzthal cross.' The cow was subsequently covered by a Mürzthal bull, and, contrary to expectation, the second calf was also a 'hybrid,' being for the most part grey, 'but possessing large brown spots like those of the Pinzgau breed.'"

Now all such cases may be interpreted, as Professor Ewart had said, "by the relatively simple reversion hypothesis." What, then, did Millais mean by saying that he hopelessly failed to induce a case of telegony, "as has every single experimenter who has tried to produce the phenomenon?" Did he mean that he had never seen a case of reversion? What did Romanes mean by saying that he had sufficient evidence "to prove the fact of a previous sire asserting his influence on a subsequent progeny?" Did he mean that he knew of direct infection, or was he referring to cases of reversion? It seems clear that the word "telegony" has been used in different senses, yet Weismann, who first used the word, makes it plain that he intended it to bear the meaning it had in Carneri's case, a resemblance of the telegonous offspring to the previous offspring. He explains his meaning as follows:—Having mentioned a few cases he says, "such cases could only be accounted for, from our point of view, by supposing that spermatozoa had reached the ovary after the first sexual union had occurred and had penetrated into certain ova which were still immature. The immediate fertilisation of the latter is rendered inconceivable by the fact of their immaturity, and the sperm-cell must have remained in the body of the ovum until the maturation of the latter, with the nucleus of which it then united in the process of amphimixis. If this occurred some time after the first of the offspring was born it might easily have coincided approximately with the second coitus, from which the fertilisation would then apparently be due. If the "infection" were proved beyond a doubt, a supplementary fertilisation of an egg-cell in this manner must be considered possible. We certainly might then reasonably ask why mares, cows, or sheep should not occasionally become pregnant without being covered a second time. *But this has never yet been known to occur*, and I incline to Settegast's view that *there is no such thing* as an infection of this kind, and that all the instances which have been recorded and discussed critically by him are based upon a misconception.[3]

[1] *Animals and Plants under Domestication*, i. p. 436. [Taken by Darwin from *Phil. Trans. Roy. Soc.*, 1821, p. 23, where the words are "some of which we observed, with much surprise, to be stained and clearly marked with the chestnut colour which prevailed in the former litter."]

[2] *Germ-plasm*, p. 385.

[3] *Ibid.* p. 385.

Thus telegony, according to Weismann, is merely a subsequent fertilisation by the first male, although Professor Ewart gives the following interpretation, which we venture to think erroneous:—"Professor Weismann thinks telegony (if it occurs) is best accounted for by supposing that some of the germ-plasm of the first sire reaches and penetrates some of the immature ova as well as the mature one from which the first embryo is developed. This germ-plasm, instead of merely serving as so much nourishment to these immature ova, according to this view, retains its independence and eventually asserts its influence on the future progeny to a second and different sire.[1]

If, then, every case of telegony investigated is reducible to one of reversion, there seem to be three possible explanations:—

1. That the reversion in the subsequent offspring was due to the influence of the previous sire.

2. That the reversion has masked or been mixed with telegony; or

3. That the reversion in the subsequent offspring after a cross was a mere coincidence.

1. There is one case which has occurred in Professor Ewart's experiments which has, I believe, been described as evidence against telegony. A skewbald Iceland pony, "Tundra," after producing a hybrid to a zebra, threw a foal to a bay Shetland, which not only showed no stripes, but was almost an exact likeness of its dam. If, however, we examine this case we see a possible connection between the crossing of "Tundra" with the zebra, and the strong prepotency she showed when mated with the Shetland pony. "Tundra" when young produced a bay foal to a pony of her own breed, she was then mated with the zebra, and produced "a dark, faintly striped, pony-like hybrid." She was covered again by the zebra, and the resulting hybrid was "so distinctly banded that it might almost pass for a zebra." She then produced the skewbald foal to the bay Shetland, and has since shown her prepotency again when mated with a bay Arab.

Now, of course it was possible that the first bay foal was begotten by an *extremely* prepotent male, but, considering how prepotent "Tundra" has shown herself to be, this explanation does not go for much. The prepotency of "Tundra" seems to have arisen after she produced her first foal, and it may not be unreasonable to connect it with her mating with the zebra.

If in all the other cases reversion in the second offspring was due to the previous sire's influence, the female must have been prepotent over her second mate, since, in spite of him, she caused the offspring to resemble her own ancestors. The prepotency, therefore, of "Tundra" first becoming evident after she had been covered by the zebra may, after all, fall into line with the other cases.

2. As to the possibility of telegony being masked by or mixed with reversion, it has been suggested that the usual experiments should be reversed, "to see if the progeny of a less highly specialised pair do not sometimes display traces of a previous union of the mother with a more highly specialised male," as "these effects could not be due to reversion."[2] There seem to be difficulties in this direction also, as the second mating, a cross, might produce reversion, and if it did not, apparent telegony might be due to a highly prepotent female.

[1] *Penycuik Experiments*, p. cxxxviii.
[2] A Note on Telegony, Xenia, and Hybrid Cology, *Nat. Science*, May 1899.

In the case of "Tundra," at first sight, the first zebra hybrid being dark, pony-like, and faintly striped, is hopeful for telegony. A Clydesdale mare, however, which had not had a previous foal, threw a hybrid to the same zebra which was quite as faintly striped as Tundra's first hybrid. The only possible point in favour of telegony seems to be that "Tundra's" first hybrid is "the darkest of all the hybrids," and whether this is due to the previous mate, the Iceland pony, it seems impossible to say; at any rate, Professor Ewart does not suggest it.

3. As to the third possible explanation that the occurrence of reversion in the subsequent offspring after a cross is a mere coincidence, there is little to be said. Reversion as a result of crossing is not uncommon, but reversion in pure-bred offspring is less common. It is strange that reversion has occurred in pure-bred offspring after a cross in many cases, but whether it is coincidence or cause and effect is not likely to be known until we have a clearer understanding of the laws which govern that complex problem, heredity.—(*British Medical Journal.*)

THE LOCAL GOVERNMENT BOARD AND NON-PROFESSIONAL INSPECTORS.[1]

BY R. E. L. PENHALE, M.R.C.V.S.

THE PRESIDENT said he would take the opportunity of thanking them for the honour they had done him in electing him as their president for this year. He accepted the position with considerable misgiving, for, in the first place, he had not been able to take a very active part in the work of the Association, such as would entitle him to the honour. Those who had preceded him had always carried out the duties with such great ability, honour, and credit to the Association, that he was quite sure they would sympathise with him in his humble endeavours to follow in their footsteps.

Mr. Bond was good enough to say at the last meeting that he (the President) was one of the oldest members of the Association, and he believed that was so.—(Hear, hear.) He was able to say that he joined it at its commencement. The first meeting was called in February 1884, when it was agreed to form an Association. At the first annual meeting the late Mr. Gregory, of Bideford, was nominated to the chair, and Mr. Penhale, of Barnstaple, was honorary secretary. It was predicted by more than one that they would not be able to keep the thing going very long, but he thought they had every reason to feel satisfied with the way it had progressed, and the position it now occupied. He was confident that it had been the means of doing the veterinary profession a considerable amount of good in this country. Instructive and practical papers had been read, and this and kindred Associations had been the means of considerably improving the social position of veterinary surgeons. It was the means of bringing together all sorts and conditions of members for the purpose of instruction and enlightenment in veterinary surgery and medicine.

There was one other object which we would like to see this Association have, and that was a combining together as a body of professional men—union if they liked. "Union is strength." It was only for some person

[1] Presidential address read at a Meeting of the Western Counties Veterinary Medical Association on July 18th.

to make a start, and they would be in a position to help forward their objects very differently than they were able to do now. They would be in a position to demand different treatment and more instruction at the hands of the large governing bodies, such as the Local Government Board and the War Office, although he believed they were likely to receive better treatment at the hands of the latter body.

He thought they had been treated very badly by the Local Government Board, especially in the matter of swine fever. No doubt they had seen Mr. Woods' article in last week's *Record*, and he spoke plainly on the matter. It was said now that after several years' efforts the disease was still on the increase. The fault was not in the regulations, but in the lax way in which they had been carried out by the various officers. It was the poor professional "tripe-washer" who came in for the fault, but it was due to the employment of non-professional men (men of no experience), and also to poor regulations. Mr. Woods, in his article, said that every inspector had a pet scheme of his own for stamping-out swine fever. That applied more particularly to his own district. He (the President) was not an inspector, but there were one or two little things he should like to see altered.

In the first place he felt the principle was wrong of appointing one veterinary inspector over the district of another veterinary surgeon. He thought that was wrong altogether. Every veterinary surgeon should be an inspector in his own practice or immediate district. These appointments caused friction and jealousy, and instead of pulling together to stamp out the disease, they only pulled away from each other. If the veterinary surgeon was capable of looking after his own clients' cattle and horses, he was capable of looking after swine. Then the veterinary surgeon, if in company with a superintendent of police, should have more power. When called upon they should have power to make post-mortem examinations or to kill any suspected animals, and the veterinary's opinion should be final. If he had any doubt he should forward a portion of the viscera to London for confirmation. But the police superintendent and the veterinary surgeon should have the power to kill, and to see that the disinfection and the burial were properly carried out. In places considered to be an infected area, the constable should be allowed to pay periodical visits as long as he thought he should do so. According to reports the Board were going to increase their staff. He did not know how they were going to employ them. If they were going to be employed as travelling inspectors that would be altogether unnecessary and wrong, because such inspectors cause a considerable amount of delay.

In conclusion he trusted that by their assistance his year of office would be interesting and instructive.

DETERMINATION OF SEX IN ANIMAL DEVELOPMENT.[1]

BY DR. J. BEARD.

THE past two centuries, said the writer, had witnessed upwards of five hundred theories of sex, more than half of which were long ago declared to be "groundless hypotheses;" and to the rest the like epithet might now be applied. Previous inquiries had proved abortive from failure to grasp the

[1] Read at a Meeting of the Royal Society of Edinburgh on July 2nd.

essence of the problem. The idea that in some mysterious way or other the sex of each individual might be determined in a particular direction during the development was doubtless responsible for failure to appreciate the true facts. Sex was predestined or predetermined for the next generation—that was to say, for the individuals of this—during the development of the individual. Thus, as an instance, all the egg-cells present in a human infant long before it itself was born had already at that early period had the stamp of sex placed upon them. Each and every one of them, if it ultimately many years later developed, would give rise to an individual whose sex in any given case had been decided long before the parent itself was born. To all appearance nothing whatever could alter or modify the ultimate destiny as to sex of any given egg-cell after a certain portion of the developmental history of the parent had been passed. Sex in all the higher animals, termed by zoologists "Metazos," would appear to have been bound up in earlier periods with the constant differentiation of gametes or reproductive cells of four categories. These were such that two of them, the female eggs and the male eggs, were formed within a sterilised (as to its own tissues) person, the female; while the development of the remaining two, the two kinds of sperms, was allotted to a similar but not identical person, the male. The two forms of eggs possessed functions different from those of the two sorts of sperms, and for this reason alone a sexual difference, a dimorphism of individuals, was bound to follow. At the present time the two forms of sperms were somewhat rarely fully differentiated, but this was known to happen in a snail, a moth, and some other instances. One kind of male reproductive cell usually underwent more or less complete suppression, the degree of this varying in different cases. The existence of dimorphic sperms was well established, having now been recorded in not fewer than thirty-six different animals of six divisions of the animal kingdom, and in man himself. Never more than one form of sperm was functional. This was of great importance, for it alone sufficed to exclude the male from any influence upon the determination of sex. It was the task of the functional sperm to bring about the effects of what Weismann had termed "amphimixis"—*i.e.*, the mixing of ancestral characters in the germ. Since it was the egg which developed, and not the sperm, the burden of providing for the continuance of the race fell upon the female, or rather upon the germ-cells, of which it is the host. The germ-cells of the female made provision not only for a new batch of female eggs, but also for one of male eggs. On the other hand, as has already been seen, the male furnished only one form of functional reproductive cell, the sperm. The determination of sex for the next generation thus lay solely with the germ-cells of the female. Under the generally accepted, but erroneous, view of the existence of only two forms of reproductive cells, eggs and sperms, but one form of individual could arise. Ordinary hermaphroditism illustrated this, for there all the individuals arising from the union of such germ-cells were alike. In all animals where two sexes were represented three kinds of functional gametes or reproductive cells were constantly required and differentiated. Of these two—the male and female eggs—arose in the female, one only in the male. Two sorts of eggs were first noted by an English surgeon, J. Dalrymple, in 1849. Since then a few cases had been added, and the discovery of two kinds of eggs in the common skate by Dr. Beard was the starting-point for his research into the question. Two categories of sperms were first reported by Professor von Siebold in 1836. Now they were known to be very common in the animal

kingdom, but, this fact notwithstanding, zoologists had hitherto with one accord ignored their existence. The actual determination of sex in the higher animals was initiated at the division of the primary germ-cells into secondary ones during the early development of the embryo containing these; it was completed by the time or very soon after the embryo had fully unfolded itself; and it had been finished in all those cells, usually termed ovarian eggs. Its manifestation was accomplished by the numerical reduction of chromosomes, *i.e.*, by that process in the formation of the ovarian eggs through which the nuclei of the latter became reduced to the half. To all appearance in plants the determination of sex would be effected at the formation of the sporemother-cells, but it did not come about in the equivalents of these in animals, the primary germ-cells. For the evidences afforded by like twins, cases where two germ-cells had undergone independent development upon one foundation, proved the primary germ-cells to be endowed with the same sexual potentialities as the embryo. The faculty of becoming hermaphrodite was confined to the female; for she alone produced two forms of functional gametes, from one of which, the male egg, she was able on occasion by anticipation to form sperms. Hermaphroditism was associated with the partial or complete suppression of one form of gamete, the male egg; parthenogenesis, on the other hand, entailed the occasional or the cyclical arrestment of one or other of the sorts of reproductive cells of the female. According to Weismann, it might become a cyclical with the disappearance of the males, and, of course, with these of the male eggs. Where there were no males the female egg underwent an isogamous or like union with a rudimentary sister, the so-called polar body. Of the problems of sex three aspects stood contrasted —its origin, its regulation in nature, and its determination. Of its origin no certain knowledge was possible, while the mode of its determination had been dealt with above. Many experiments had been made upon "the origin of sex" during development, thus those of Born on frogs, and Mrs. Treat upon caterpillars. Their results are vitiated by the assumption that the animals used were either of indifferent sex or hermaphrodite, and what they had really tried to do was to regulate sex, often under utterly abnormal conditions. The regulation of sex in nature was, as Hensen and Duesing surmised, self-adjusting. Like the determinaion it lay with the female. The total females of a race occupied the same position towards the regulation of sex as the individual female did towards its determination. The basis of the principle was simply the preponderance of the males in the earlier and of the females in the later offspring. An increment in the race was effected by increasing the number of offspring, and with these the number of females. The effect of this began to manifest itself in the third generation. It should be especially noted that the number of males was only of importance for the particular generation in which they occurred. For just as with the bee-hive the number of drones had no bearings whatever upon either the absolute or the relative number of the two sexes in the following generation, so also with other animals and man himself, the number of males in any particular generation did not in the least determine either the absolute or relative numbers of the two sexes in the next. All interference with or alteration of the determination of sex was apparently absolutely beyond human power. To hope ever to influence or modify its manifestations would be not less futile and vain than to imagine it possible for man to breathe the breath of Life into inanimate matter. For the workings of Nature in sex merged in her revelations of Life itself.

TICK-HEARTWATER EXPERIMENT.

(*From the Agricultural Journal of the Cape of Good Hope, published by the Agricultural Department.*)

NOTES on an experiment conducted in the latter part of 1899 with the view of ascertaining if any relation existed between tick parasites and the heartwater disease of sheep and goats, were published in my annual report for last year. The results of that experiment indicated, though not in a satisfying degree, that Bont ticks (*Amblyomma hebræum*, Koch) fed in one stage on heartwater diseased animals may in their following feeding stage transmit the disease to susceptible animals; and, secondly, that Red ticks (*Rhipicephalus evertsi*, Neumann) under similar circumstances do not transmit the disease. In this experiment two goats only were exposed to the presumed possible sources of infection,—one being exposed to the attack of adult Bont ticks nourished in the nymphal stage on diseased animals, and the other to adult red ticks nourished in the nymphal stage on the same diseased animals. The first goat developed a fever after a few weeks, which if not the fever of heartwater, at least resembled the fever of that disease, whilst the second goat gave no noticeable reaction.

The 1899 experiment was followed in the first half of the year being reviewed in the present report by an experiment on exactly the same lines, but conducted more systematically and with a greater number of animals. A preliminary report on the work whilst it was still in progress was published in the *Agricultural Journal* for May 24th.

Plan of the Experiment.—To clearly understand the experiment, it is necessary to bear in mind that the ticks concerned, the Red tick and the Bont tick, have three distinct stages, during each of which they take blood from an animal. In the first or larval stage the ticks are mere specks, and naturally crawl on to the animal from grass tops or other vegetation. The Bont larvæ attach themselves almost anywhere, whilst the Red larvæ generally wander about until they get deep into the ears. In a few days both kinds have stretched their skins to the utmost with the blood imbibed, and then must perforce cease feeding. The larval Bont lets go its hold and drops. The larval Red retains its hold. After a varying interval of quiet, which is far longer in the case of the Bont than in the case of the Red, both shed their larval skins and become voracious again. They have now entered the second or nymphal stage. The Red nymph settles down at once near its old skin in the ear; the Bont nymph proceeds to find a host. In due course the two kinds of ticks become engorged again and then *both* drop off, and after resting awhile shed their skins as before, thus entering the third or adult stage. The Red adult seeks the anal regions by preference; and the Bont adult, the belly, the udder, and genitals. The third and last feed taken, the females of both kinds drop off, secrete themselves, and in their hiding-places lay eggs for the succeeding generation.

The plan of the experiment was to get large numbers of both kinds of ticks engorged on animals infected with heartwater, and which these ticks dropped naturally, to collect them, care for them until after they moulted, and then to liberate them on healthy animals.

Infection of the Ticks.—Bont and Red larval ticks for the experiment were obtained by the incubation of eggs at the office. The mother Bont ticks were from cattle in the Fort Beaufort district, and the mother reds from one of the goats used in the previous experiment. These larval ticks were taken to Mr. Llewellyn J. Robert's farm, Cottesbrook, in the Fort Beaufort

district, in February for the purpose of carrying them through their first feeding stages on heartwater diseased animals. Cottesbrook is a farm which when stocked with sheep some years ago became badly affected with the malady, and hence was a site which I was certain would be favourable for the initial part of the experiment.

Four old Angora ewe goats to serve as hosts for the ticks had by previous arrangement been procured by Veterinary-Surgeon R. W. Dixon and infected with heartwater by intravenous inoculation of blood from a diseased animal. These goats were numbered 1, 2, 3, and 4. No. 1 had been given 20 cc. of diseased blood, and the others each 10 cc. There being a slight doubt in regard to the virulence of the blood used by Mr. Dixon, the animal he drew from not having been as ill from its fever as it might have been, an opportunity was taken to reinoculate two of the animals, Nos. 2 and 3, with blood drawn from a sheep which chanced to be dying from the malady on the next farm. Mr. Roberts conducted the operation, and injected 10 cc. into the jugular of each. The first inoculations were made February 23rd, the second February 28th.

All four goats reacted to the inoculating, the temperature rising abnormally in twelve to fourteen days. Ordinarily the fever of heartwater begins to be manifest about the ninth day from intravenous inoculation with virulent blood. Goats 1 and 4 passed through several days of fever without manifesting their illness in their appearance or by lack of appetite. The temperature of 1 reached the maximum, 105·1°, on the seventeenth day, and that of 4 the maximum, 107°, on the fourteenth day. Both these goats were later turned out on heartwater veld. No. 4 died some months after from causes not determined; 1 is still alive and perfectly healthy. Goat 2 became feverish on the twelfth day, evidenced severe illness by its hair, dejected appearance, and lack of appetite on the day following, and on the next day succumbed very suddenly. The maximum of its temperature was 106·1°. An examination of the organs showed that its death was unmistakeably due to heartwater. Goat 3 became ill on the thirteenth day, and until the eighteenth seemed in danger of dying at any moment; she then rallied, but died from exhaustion the following week when turned out on to the veld. During all but the first four days the animals were confined in a stable and fed on the foliage of veld bushes. Owing to its stone walls, a room overhead, good ventilation, and the absence of sun, the stable was cool all of the time, which fact doubtless had its effect on the temperature of the animals.

All four goats were infected with larvæ of the Bont, and goats 1 and 3 with larvæ of the Red. To ensure having some of the ticks engorging themselves during the fever period, the larvæ were not all applied at once, but numbers put on every other day.

Recovery of the Ticks.—The Red ticks when they dropped as engorged nymphs, which occurred in about two weeks from the date of their application, were caught in cloth caps tied over the ears. The caps were removed and examined four times a day. The Bonts, as engorged larvæ, began to drop in about four days, and were caught in cloths, suspended beneath each animal, and held in place by tapes tied over its back. The cloths were examined hourly, when the ticks were dropping, from early morning until late at night. The first catch cloths used, or "pants" as we graphically term them, were crude affairs, with hemmed openings for the insertion of the legs. In later experiments, under way at the time of writing, strongly made garments, which enclose the animal, legs and all,

from neck to tail, are used, and are more satisfactory; when they are well fitted and properly tied there is no trouble from the passing of the fæces or urine, and practically all the ticks are caught uninjured.

The animals were fastened in sets of stanchions while the ticks were being fed and recovered, and thus were prevented from nibbling the cloths and tapes. The stanchions were fashioned much like those used for holding milch cows to their stalls in countries where cattle are stabled. The neck of the animal is held between two uprights, which are far enough apart not to irritate the skin of the neck, but too close together to permit the head to be withdrawn. The goats could lie down and stand erect without any discomfort, and, after the first day, so took the neck confinement to be in the ordinary course of affairs that they evidenced an anxiety to get back to the stanchions whenever released.

According to whether Bont or Red, the animal on which fed, and the time of dropping, the ticks recovered from the goats were classed as follows:—A, Reds from goat 1, and B, Reds from 3, during the fever; C, Bonts from 1, and D, Bonts from 3, during the same period; E, Bonts from 3, during first two days after fever; F, Bonts from 4 during fever; and G, bonts from 2 taken just after this animal died.

Tick Infection of Susceptible Goats.—The scene of the experiment was transferred to Capetown after the twentieth day. The various lots of ticks were placed in an incubator heated to 85°—92° F., and retained there until they moulted, and were thus made ready to feed again, the Bonts as nymphs and the Reds as adults. They were then carefully separated from their cast skins, and placed in new clean boxes.

Cross-bred young Angora ewe goats for the continuance of the experiment were procured from Stellenbosch, a district far removed from the heartwater section of the Colony; and as the animals were reared on the farms from which they came, no doubt was felt on their being susceptible to the disease. An old shed in the heart of Capetown was fitted up for the reception of the goats. Stanchions were erected at each end, and one end reserved for Red tick infections. and the other end for Bont tick infections. The shed was commodious, and fully 35 feet of space separated the two sets of stanchions. Eight goats, numbered on the horn from I. to VIII., were brought into the shed at first, and two others, numbered IX. and X., about a fortnight later. I. to IV. were put at the Red tick end, and infected with that species, I. and II. with lot A, and III. and IV. with lot B, all dating from April 6th. Goats V. to X. were kept at the other end, and infected with the Bont tick, V. and VI. with lot C on April 6th, VII. with lot D on April 7th, VIII. with E on the 10th, IX. with F on the 19th, and X. with G on the 22nd. The liberations were all made in cloths such as have been mentioned. Most of the ticks attached themselves within twelve hours. The Reds showed a disposition to roam, and doubtless some specimens got on to another goat than the one for which they were particularly intended. The Bonts, too, wandered somewhat, but much less than the Reds, and it is doubted that any went farther than to the next animal in line. It is possible, but not probable, that Reds crossed the shed to the Bont end, and Bonts to the Red end; if any such migration took place it escaped our detection, and we were on the watch.

All due precautions were taken to eliminate possible sources of infection other than the ticks. Nothing other than my person and the ticks that had been in contact with diseased goats was allowed to come into the shed.

The clothes that I wore when working at Cottesbrook were left there, and hence it was unlikely that I brought any infection. The ticks had shed their skins, mouth parts and legs included, since having come from the diseased animals, and externally were seemingly incapable of imparting disease. Oat forage and bran purchased locally were used as food. The previous use of the shed was at the store place for builder's material.

The shed was not an ideal place for the conduct of the experiment, because of its variable temperature. It is a corrugated iron lean-to against a fence, loosely constructed, and without windows, and exposed the whole day to the full force of the sun's heat. The temperature readings of goats I. to XI. are not therefore directly comparable with the temperature readings of goats 1 to 4, the latter, as has been stated, having been kept in a cool stable.

Results of Bont Tick Infestation.—About seventy-five specimens of the Bont tick attached to V. goat ; two hundred to VI. ; one hundred and fifty to VII.; and one hundred and seventy-five each to VIII. and IX. Lot G ticks applied to X. consisted of twelve specimens only, and of these ten attached. The Bont tick nymph is a tiny creature, small in comparison with the adult Red tick, and even 200 specimens seemed few when seen on the animal until engorgement was nearly completed. Nearly half of the attachments were about the feet ; the remainder were scattered over the body, but chiefly along the median line of the ventral surface, and under the thighs.

Effects from the attack of the ticks became distinctly evident after an interval of eleven to fourteen days from the application of the specimens. Each goat in its turn was made ill, and showed its illness not only by its rise of temperature, but by failing to partake of proffered food and drink, and by its dejected appearance and assumed positions. Number V. gradually regained its normal appetite after a fever of several days' duration, in which its temperature rose to 107°. VI. survived an attack in which for a few hours its temperature registered above 108°. VII. went through the course of high fever, but succumbed on the twenty-second day. VIII. behaved similarly, dying on her twenty-first. IX. had a short and distinct fever, but was hot prostrated as were the other animals. X. was made exceedingly ill, had a long fever, and never fully recovered. Dr. Hutcheon, the Colonial Veterinary Surgeon, viewed the animals several times during their illness. His assistant, Dr. W. Robertson, conducted the post-mortem examinations on the two that succumbed, and attributed death in both cases to typical heartwater. It should be noted that the ticks responsible for transmitting the disease had all left the animals before these were taken ill.

At my request Dr. Robertson drew blood from VII., VIII., and X. as their fevers began to abate. 10 cc. from VII. was injected into the jugular of I. on April 26th, and on May 6th this animal died of heartwater. 10 cc. from VIII. was put into III., and this animal died from the disease May 8th. The blood from X. was used to infect a small sheep, but unfortunately this animal died from other causes before there was time for heartwater fever.

It is worth noticing that the intensity of the fever in the various cases produced by the ticks appeared to vary with the intensity of the fevers in the animals on which the infecting ticks had been fed. The ticks from I., an animal which was not thrown off its feed by its illness, produced severe but not fatal cases (V. and VI.) ; those from 3, which was exceed-

ingly ill, produced fatal cases (VII. and VIII.); those from 4, an animal not so ill as I., although it showed a higher temperature, produced a rather mild case (IX.); whilst those from 2, which died, though only ten in number, produced a long and very severe case (X.) These apparent correlations may. of course, be mere coincidences.

Results of Red Tick Infestation.—About 125 specimens of the Red tick attached to goat I., 150 to II., 100 to III., and 60 to IV. The sexes were present in approximately even numbers. The species of tick is remarkably partial in the adult stage to the hairless region about the anus and vagina, and all of the specimens employed there located themselves. Owing to the size of the ticks, and their occurrence over so limited an area, the infestation seemed extremely heavy, especially as the females became engorged; and, indeed, infestation by this kind of tick to the same degree on the veld is improbable. Although fewer specimens attached to III. and IV. than to I. and II., the former goats were presumably exposed to as much danger of infection since the specimens they received came from a more badly diseased animal.

The experiment was continued until the 5th of June and the temperatures taken twice daily, morning and evening, during the period. Two of the animals, I. and III., as has already been explained, were inoculated with virulent blood on April 26th, and hence the experiment closes for them with this date. No symptoms of disease suggestive of heartwater, other than abnormal temperatures, occurred in any of the goats.

For purposes of study and comparison, the temperature records of all the goats confined in the shed during the period of observation are submitted. In addition to the records of the Bont tick and Red tick goats, the records of two goats not heretofore mentioned are given, in one case dating from May 2nd, and in the other from May 12th. One of the animals, XI., was not molested with ticks; blue tick larvæ, indeed, were applied to her, but none became attached. The other, XII., served as a host for a few nymphal tortoise ticks (*Amblyomma devium*); these ticks as larvæ had fed on a quail, and there is no reason to suppose them to have been pathogenic. Unfortunately, this goat aborted on the eighth day temperatures were taken, yet except for a few days immediately subsequent to this accident the temperatures may be safely accepted as normal to an animal in health, under the circumstances of the season and confinement.

Abnormally high temperatures rule for goats I. to VI. throughout. In the case of the Bont tick infected goats, no evidence of disease was shown by the temperature (or otherwise) before the eleventh day, and we may assume that such would have been the case in I. and IV. Prior to the eleventh day the temperatures of the two lots of goats should show much uniformity, but a comparison shows the temperatures of the Red tick goats to have been relatively higher. The difference is most marked in II. and IV., the average morning temperatures of which for the period are respectively 103·5° and 103·4°. Heartwater can hardly be held to account for these high temperatures. Suspiciously high temperatures rule in I., II., and III. between the twelfth and fifteenth days, April 18th to April 21st, the period when heartwater fever would be expected to occur from tick infection, as shown by the Bont tick infected goats. The temperatures of IV., although constantly much above the normal, at the same time are not comparatively high. But the days in question were oppressively hot, with hot nights, and this fact, in connection with the circumstance that the

animals were confined in a close shed, doubtless had considerable effect on the body heat, and this effect we may rationally assume to have varied in degree with the different animals. It will be observed that during the same period the temperature of VIII. was also very high, and that it fell before the true fever rise came on some days later. All in all, therefore, it is unsafe to conclude that the high temperatures of I. to IV. for these days was due to the disease. From May 12th to the close the records of II. and IV. may be compared with those of XI. and XII., and it then becomes more evident that the temperatures of both are erratic and often abnormally high. XII., be it remembered, was practically free from suspicion of infection, and XI. was absolutely so. Therefore it seems clear that the Red tick animals then surviving, II. and IV., and probably I. and III. as well, had been affected in temperature by some factor present in them, but not present in XI. and XII. The data of the experiment is insufficient to determine whether or not this factor was heartwater infection. However, Mr. R. W. Dixon, of the Colonial Veterinary Staff, who has been paying close attention to heartwater for several years, with the object of discovering a safe means of immunising animals, had studied the records by request. He does not think the high temperatures can be wholly attributed to the heated atmosphere of the shed, and suggests that the goats were slightly infected with heartwater by the ticks. In his experimental work, he said he had somewhat similar temperature records from animals which, under his manipulation, were suffering from very mild heartwater. These milk attacks, he further said, gave the animal little or no protection from infection later on.

It will be noticed that fever developed in III. on the ninth day after inoculation with virulent blood from the Bont tick infected animal, and that death came on while the fever was still high. Such behaviour is typical of a case of heartwater from intravenous inoculation with a lethal amount of virulent blood, and, as already stated, the post-mortem revealed heartwater. Goat I., from a similar inoculation, gave a somewhat different reaction. Fever set in on the third day after the inoculation, ran a course and abated, and then rose and culminated in death on the tenth day. At the time it was feared that the Red tick infection may have been a factor in causing the first rise of temperature, but recently it has been learned from Mr. Dixon that such a course of fever following close on inoculation has not been infrequent in animals he has infected. Therefore, the infection of both I. and III. affirms simply (1) that the animals were susceptible to the disease, and therefore would have taken it from the Red ticks were these able to communicate it, and (2) that the disease produced by the Bont ticks was certainly heartwater.

Conclusion.—The experiment conclusively demonstrates that a very limited infestation by nymphal Bont ticks which fed in their larval condition on a heartwater diseased animal will produce the disease in a severe form in susceptible animals. And (2) that even a gross infestation by adult Red ticks which fed in their earlier stages on a heartwater diseased animal will not produce severe cases of the disease in susceptible animals. The experiment indicates but does not prove that the Red tick may, by gross infestation by specimens from a diseased animal, transmit a very mild and unimportant form of the malady.

Incidentally, the experiment affirms to some degree the findings of the Veterinary Department to the effect that the disease is not contagious.

The animals employed in the latter half of the experiment were confined in the one shed, were fed with the same kind of food, were watered from the same buckets, were handled by the same men in the same clothes, and their temperatures taken twice or more daily with the same thermometers; yet all the positive cases of the disease appeared only after a definite time from the application of infective ticks, and the two animals which received no suspected ticks gave no indication of disease whatever.

It has not been shown that the larval ticks produced from mothers which engorged on diseased animals may communicate the disease; that is, that the disease is communicable through the egg stage. But the experiment of a year ago, mentioned in the opening paragraph, may now be accepted as showing that the disease is transmissible from nymph to adult.

[We are of opinion that in all probability "red braxy" and "water braxy" in sheep are analogous diseases to "heartwater disease," and would strongly advise the farmers in Scotland to initiate a careful investigation of the matter. It will be remembered that the late Principal Williams in 1880 stated that "louping-ill" was due to the introduction into the system of sheep by the tick of an organism which caused nervous symptoms, but for a long time pathologists sympathetically smiled when they heard this statement. Now they blame fleas, flies, mosquitoes, ticks, and other vermin for most diseases under the sun.—ED. *V. J.*]

ON BOVINE AND HUMAN TUBERCULOSIS.[1]

GENTLEMEN—The remarks which I am about to make are in reference to the discussions which have been going on in London on the question of Tuberculosis, human and bovine. Amongst these the points raised by Lister have justly received the principal consideration. I observe also in the Berlin papers that stress is laid on the fact that we have no corresponding experiments on man, and that we ought therefore to suspend our judgment in the meantime. I have had the honour conferred on me of being appointed a member of the Government Investigation Commission; I must ask you to wait till the publication of their researches takes place, and this will depend on official sanction. I myself cannot move in the matter, but I will say this, that at the Conference which was summoned by the Imperial Health Bureau, and at which all the principal tuberculosis experts in Germany took part, the necessity of making further experiments was one of the chief subjects of discussion. A whole series of fresh demands were made, some of which you, gentlemen, may think superfluous, but, in view of the extreme importance of the matter to be decided, it seemed necessary to give a clear access to every possibility of observation.

I may here state that the preparations resulting from the experiments made specially in the Veterinary College here under the direction of Professor Schütz, and controlled by Mr. Koch, were exhibited in the Conference Chamber, and that, in my opinion, there could be no doubt as to their sufficiency for the purpose for which they were intended. It was clearly demonstrated that infective material drawn with the utmost precaution from the products of human phthisis had produced in the animals

[1] Address delivered by Professor Rudolf Virchow to the Berlin Medical Society 24th July 1901.

experimented on *no* phenomena which could be compared with bovine tuberculosis.—(German, *Perlsucht;* English, *Grapes*).

As for proof of the contrary, namely, that bovine tuberculosis has not been produced experimentally in man—of course that could not be expected—we must wait for further opportunities. But in this connection I will say that Mr. Koch has perhaps gone too far in the exclusion of all those cases in which possibly transmission of bovine tuberculosis to man has been brought about by *food*. We have, as a matter of fact, had from time to time such cases in the Charité (Hospital), and some preparations have been collected in which there were present the very unusual phenomenon of peritoneal tuberculosis and, what is more, masses of luxurious growth (proliferations?), such as do not usually occur in the human subject, we have regarded all such cases with the very greatest suspicion, and do still regard them. I therefore consider it possible that Koch's denial may itself in the future be denied. On the other hand, I have no hesitation in accepting Koch's statement based on his new experiments, namely, "I maintain with much satisfaction that human tuberculosis differs from bovine tuberculosis, and cannot be transmitted to cattle." Here we have two statements combined, namely, the difference of the two diseases and the question of non-transmissibility. As to the latter, I have already told you that the specimens placed before you tell their own tale. As to the former, I must mention the very curious circumstance that my old thesis, which was intended to prove that the two diseases differ, was for a long time treated by the Koch school with a certain contempt; I as a patient man submitted to this judgment, but of course I cannot say that I am surprised to learn that Mr. Koch has now become convinced that the two diseases *are* different. For my own part I could never understand how any one could assert their identity.

In this connection I should like to remark that to my way of thinking we cannot give the name of tuberculosis to any manifestation in which tubercles do not arise in a form in which they prove themselves pathologically and anatomically to be real tubercles; but not everything in which tubercular *bacilli* present themselves is to be called a tubercle straight away. This confusion has, in my opinion, caused a great many of the difficulties which have arisen in the minds of the public, and more especially in the minds of medical men, for if no decision could be arrived at as to what was to be called a tubercle, it was certainly impossible to give permanently any clear and generally intelligible statement. Perhaps I may here once more state emphatically that my conception of a tubercle is not merely a thing in which there are tubercular bacilli, but one which is composed of cells which we may call tubercle-cells, that is to say, that in the tubercle an organism is present, a growth, something growing or grown out of the body itself, arising, it may be, at the instigation of tubercle bacilli. But the tubercle bacilli themselves are no constituent element in it; the constituent element must be cells which have proceeded from the living body itself. You will remember that I have repeatedly emphasised the points of difference between mere bacillary products and genuinely tubercular products. This distinction has therefore nothing novel for me; I will only say with reference to the long period which has elapsed—you must reflect that this deception has, as Koch now admits, persisted for ten years—if other ten years are not to be lost, people must make up their minds to keep the two things carefully apart, and place in the foreground of obser-

vation only the really *pathological* tubercles, and not the mere *bacteriological*. Now the difficulty in the way of a clear understanding lies essentially in the fact that it was supposed we could call a formation supplied with the relative bacteria a tubercle without more ado. In this way not only grapes in cattle and lupus in the human subject have come into the foreground of observation, but, what was more startling, even the anatomical wart took its place there, because it chanced now and again that in the microscopic section taken from an anatomical outgrowth of the skin a tubercle bacillus was found. This way of looking at things must at once be altered. It must be clearly understood that there are not only bacillary tubercles and bacillary hepatisations, but also *non*-bacillary, and that not everything in which there is a bacillus can straightway be called a tubercle. We must rather regard every tubercle as an organic formation which has grown out of components of the body. I wish specially to emphasise this point.

As I have now the honour to have been elected a member of the Investigation Commission which is to have the control of further experiments, I promise you that I shall endeavour to lay the utmost stress upon this distinction, in order that you, gentlemen, may not be trammelled with the difficulties which have lasted for the past ten years. It will be a good thing for the public, and I for my part shall rejoice, if it can be clearly proved that the tubercle bacilli of the ox can *not* pass into the human body through the medium of milk, flesh, and what not, so frequently as has been commonly supposed. I have always thought that the fear was exaggerated. I have never allowed myself to be deterred from drinking milk or eating meat by the possibility of their containing a bacillus. I have always been of opinion that it was not a question of one bacillus or two, but that only when a certain quantity of bacilli were absorbed was there danger. But this question of quantity has scarcely ever yet been treated by bacteriologists. They act as if, when a typhus or cholera bacillus is found, it were sufficient to produce immediately endless millions of its kind. We must be a little more cautious, gentlemen, and I promise for my part to see that the *anatomical* tubercle gets its full rights, and that in future we shall take care not to confuse things anatomical with things bacteriological.

In the *Berliner klinische Wochenschrift* (August 2nd) F. Hueppe criticises Dr. Koch's statements as to the non-identity of the bacilli of bovine and human tuberculosis very severely. He points out that Koch and Schütz found (as some American investigators had found some years back) that human tubercle bacilli were not disposed to grow in cattle, and the former attempted to show that conversely the bacilli of bovine tuberculosis were not infectious for man. He considers that the experiments were not carried far enough to justify any conclusions. "Can one form any certain opinion from such negative inoculation experiments, whether the kinds of micro-organisms are different or identical?" he asks; and answers "No." Mafucci and Koch made the same mistake when they claimed that the bacilli of avian and mammalian tuberculosis are different organisms. These bacilli had certain characteristic culture differences, but these differences were due to modification of the culture media as well as varieties of environment. Fischer and Hueppe succeeded in producing these changes and variations artificially, and in grafting avian bacilli on to mammals, and

mammalian bacilli on to hens. Nocard has shown that monkeys at times become infected with true avian tuberculosis. "And now Koch expects the sacrifice of intellect from us, and asks us to believe that the varieties of human and bovine tuberculosis which stand nearer to each other culturally, or, still more, are not distinguishable from one another, must be *toto cælo* divided from one another because they do not respectively attach themselves in his animal experiments."

After discussing the pathological conditions of the nodules found in bovine tuberculosis, and also found *in situ infectionis* by Koch, he proceeds to describe how he had noticed many years ago that bacilli in bovine tuberculosis nodules, stained in the usual way, exhibited slight differences in form from human bacilli, and, indeed, were more like human *Lepra bacilli*. He looked upon these changes in form from the first as due to a parasitic form of adaptation to the peculiar soil of the cattle. He says that we have experiments which inform us better than Koch can. Although it is for the most part difficult to alter the differences of the bacilli of mammalian and avian tuberculosis, it is quite the reverse with the various kinds of mammalian tuberculosis. The bacilli behave in culture absolutely alike, and the carrying over such cultures to different suitable animals—for example, to guinea-pigs or rabbits—is easy; these animals apparently take the bacilli up readily and equally well. Only one sometimes needs to use greater quantities for one animal than for another. Thus it is only a question of virulence, and he especially refers to the work of Weleminsky on this subject. We are therefore justified, he says, in concluding that even in the bodies of our small experiment animals a complete equalising of the original slight differences takes place at once. And this is only possible if the causal organisms of mammalian tuberculosis, and especially of human and bovine tuberculosis, are identical as far as kind is concerned. He regards a natural infection as consisting of an infecting parasite (in the present case the tubercle bacillus, or rather the parasitic form of a pleomorphic fungus), which in suitable condition meets a susceptible individual of a susceptible race. He further believes that bovine bacilli can affect the human subject, and points to one method of infection by means of an intermediate host—the pig. As far as the dangers of tuberculous milk are concerned, he points out that Weleminsky has shown that micro-organisms taken in with fluids are more likely to be attracted to carious teeth and tonsils than to the intestinal wall. There is a danger of the milk introducing its organisms through the tonsil, and also from the surface of the tonsils, &c., to the respiratory passages directly, and he considers this to be a great danger. Turning again to Koch's arguments, he points out that primary intestinal tuberculosis is by no means so rare as Koch stated. Including cases of tabes mesenteriæ, Heller puts the incidence at 50 per cent. of tuberculous affections of children. He finds that when the cows are kept on the marshes, as in the German Alps, raw milk being drunk indiscriminately, far less tuberculosis appears than when the cows are cooped up close to one another in stalls, and are obviously likely to carry the infection to one another. He concludes by dealing with the subject of general prophylaxis in a broad way, and calling attention to the importance of attacking both the bacilli in all their forms and the predisposing causes in the human subject.—(*British Medical Journal*, 7th Sept.)

In the *Berliner klinishe Wochenschrift* for September 2nd, pp. 894-6, Professor P. Baumgarten, of Tübingen, has contributed an important paper on the relation of bovine and human tuberculosis, taking the contrary side to Dr. Hueppe, and supporting in great measure Dr. Koch's position. After referring to experiments made in his laboratory by Dr. Gaiser in 1893, in which two calves had bovine and human tubercle bacilli respectively injected into them, with the result that the former caused general miliary tuberculosis, and the latter produced no effect, he supplies details of some unintentional experiments performed twenty years ago, which more or less satisfactorily supply the missing link in Dr. Koch's train of argument at the recent Tuberculosis Congress—that is, a negative effect from injecting bovine tubercle bacilli into man. A physician now dead, Dr. Rokitansky, in a hospital with which Dr. Baumgarten was connected as pathologist, had under his care several patients suffering from generalised malignant disease, both carcinoma and sarcoma, which being inoperable meant a soon or late fatal termination. Reflecting that tuberculosis rarely occurred with sarcoma or carcinoma, and on a similar principle to that by which erysipelas cultures have in recent years apparently exerted a remedial effect on similar growths, he hoped by injecting tubercle bacilli into these patients to favourably affect the malignant tumours. As at the time no suitable cultures of human bacilli could be obtained, and as those of the ox were believed to be identical with them, bovine tubercle bacilli were employed. Large quantities were injected under the skin of some half-dozen patients with absolutely negative results. In some cases small pustules developed at the point of inoculation, which at first were found to contain a few tubercle bacilli, but when the wounds healed these were no longer to be found. Dr. Baumgarten, at the necropsies of some of these patients, found, after the most searching microscopical examination, that the small scars at the site of the injections were entirely free from tubercles or tubercle bacilli. Although most of the patients survived the inoculation several months, no development of tubercle occurred. In meeting the obvious objection that the patients suffering from sarcoma and carcinoma were insusceptible to infection by tubercle bacilli, Dr. Baumgarten remarks that, as is now known, tuberculosis and carcinoma and tuberculosis and sarcoma are not rarely present in the same persons, and even indeed in the same organs, arguing from this that the subjects of malignant disease are not specially resistant to tubercle bacilli. He accordingly agrees with Professor Koch that bovine tuberculosis cannot be transmitted to man, but differs in believing that this is due not to their being different diseases, but to the modification the primitive tubercle bacilli have undergone by being grown in different hosts. That is, he emphasises the modification which bacilli undergo in accommodating themselves to the conditions of life in different animals, and that these modifications in the case of the bacilli of bovine tuberculosis have rendered them harmless to man. This theory, which was also referred to by Lord Lister and Professor Nocard at the recent Congress, is, he considers, more tenable than that which ascribes different kinds of bacilli to different species of animals, mammalian and other. Although, then, he believes in the harmlessness of bovine tubercle bacilli for man, he would not have any relaxation in the precautions against the possible transmission of the disease by food, at any rate until after the results of extensive experiments are known, and the existing regulations for the prevention of such a possibility must not be regarded as in any sense quixotic.—(*British Medical Journal*, 7th Sept.)

SCIENTIFIC RESEARCH: THE INDISPENSABLE BASIS OF ALL MEDICAL AND MATERIAL PROGRESS.[1]

BY GEORGE BAGOT FERGUSON, M.A., M.D. B.CH., OXON., SENIOR SURGEON TO THE CHELTENHAM GENERAL HOSPITAL, AND TO CHELTENHAM COLLEGE.

The Medical Standpoint of To-day the Result of Scientific Research.

Pondering over medicine as it stands to-day, the main fact that strikes me about it is, how much more it owes to the biologists and to the men of pure science than to the so-called practical men. The practical man is indispensable, but he is not like the great biologists, a High Priest of the Arcana of Nature. The cell theory, for example, lies at the very foundation of of modern medicine, and this theory certainly originated entirely with the biologists. The term "cell" was first used in reference to plants in the seventeenth century (1665) by Hooke, who, with his contemporaries, Grew and Malpighi, distinctly foreshadowed the cell theory of modern times. The cell nucleus was actually seen and drawn by F. Bauer so long ago as 1802, though it was not before 1831 that it received that name from Robert Brown, the botanist. Little notice, however, was taken on the subject before the publication of Schleiden's paper on Phytogenesis in *Müller's Archives* for 1838, in which he asserted that "every plant is an aggregate of individualised, independent, separate beings—namely, cells." Theodor Schwann, also in the next year, made a similar assertion regarding animals in his *Microscopical Researches*, of which, and of Schleiden's paper also, the Sydenham Society published a translation in 1847.

Schwann was first impelled to his conclusion by noticing the extraordinary resemblance between the microscopic structure of the chorda dorsalis of the tadpole and that of the onion and of certain pollen matrices. Schwann, however, fell into error in asserting that cells arose spontaneously, and here he was set right by Barry in 1838, and later by Goodsir, who established the direct descent of every cell from a pre-existing one. This is Virchow's "continuity of life," his *omnis cellula e cellulâ*, the aphorism which he offers us in the place of Harvey's *Omne vivum ex ovo*." That *punctum saliens*, the nucleolus, was first noticed by Schleiden, though it was Schwann who gave it that name. Next came Hugo von Mohl in 1846, who recognised what we now call protoplasm in the cells of plants; the identity of which with a similar substance in the cells of animals was soon afterwards proved by Cohn and Remak. To Schwann a cell without a wall and a nucleus was no true cell, but Leydig in 1856 adduced pus and mucus corpuscles as instances of walless cells, and so step by step the ground was cleared for the now historical Cellular Pathology of Virchow, which influenced so profoundly the medical thought and practice of forty-three years ago. To Virchow as to Schwann a cell wall was a necessity, though this idea was soon afterwards dispelled by Lionel Beale, and independently by Max Schultze in 1861. To Max Schultze we owe not only the extension of the term protoplasm (the bioplasm or living matter of Beale) to the contents of animal cells also, but the best description of an animal cell, namely, that it is a protoplasmic mass containing a nucleus; though even this last, we may add,

[1] Extracts from President's address, delivered at the Sixty-ninth Annual Meeting of the British Medical Association.

is non-essential. All this work on the cell theory gave the original inspiration to Pasteur, and Pasteur leads up to Lister, of whom I will forbear to speak, for he needs no speech, though he might well say of himself, *Quæ regio in terris nostri non plena laboris?* I may mention this significant fact, however, that, visiting the Polyklinik in Vienna two years ago, I noticed that the only name of a contemporary Briton inscribed on its façade was that of Lister. The cell theory further leads up to bacteriology, the most imposing and the most impressive department of medical biology.

Now bacteriology, which originated with the labours of Cohn and De Bary, rests on cultivation and staining; and if year by year more and more of the germs of disease are recognised, it is because improved methods are constantly being perfected for colouring and making visible these formerly invisible entities. The modern practice of staining, so essential to the physician and surgeon of the day, began with the botanists Göppert and Cohn, who used carmine in 1849, and here again the pure scientists showed the way to the practical investigators of medicine, such as Gerlach of Erlangen, who first applied the method to anatomy. In 1871 and later Weigert succeeded in demonstrating nuclei by ammoniated carmine, and cocci by methyl violet. In 1872 Eberth and Wagner began the use of that admirable stain, hæmatoxylin. Looking through some old preparations of mine not long age, I found that most of those stained with aniline dyes were faded, but those stained with hæmatoxylin were still bright and good. In 1879 Ehrlich enunciated his remarkable generalisation that the basic coal-tar colours (like fuchsin, methylene blue, and methyl green and violet) stain nuclei; whilst the acid ones (like orange-green) stain plasm, and the neutral ones (like rosaniline-picrate) stain special cell contents. Weigert, later, found that acid fuchsin was specially useful in the study of the nervous system, and Strasburger, the botanist, used the basic methyl-green to demonstrate those mitotic figures which are of such importance in embryological work, and have afforded a basis for the wide generalisations of the biologist Weismann. Nor need I pursue the matter much further than to state that in 1882 Koch, who had previously devised the method of cultivation of bacteria, first stained with methylene blue and thus discovered the *Bacillus tuberculosis*, the consequences and far-reaching results of which discovery are well known to all of you. That I have said so much on this point is to strengthen my contention that the true basis of modern medicine is essentially scientific. As an admirable instance of the kind of debt that modern medicine owes to bacteriology, of its power and promise, as also of the extreme difficulty and complexity of the subject in its recent developments, I cannot do better than to advise the re-perusal of Dr. Horton-Smith's Goulstonian Lectures on Typhoid Fever.

I spoke of the antitoxins as among the most valued resources of remedial art, and it is to strictly scientific investigators, to such men as Loeffler and Roux, Behring and Kitasato, Haffkine and Yersin, that we owe these great discoveries.

Soon, I believe, we shall possess antitoxins for most of the febrile and infective diseases as undoubtedly effective as is that one now so successfully employed against diphtheria. Personally, I place much faith in the antityphoid inoculations of Professor Wright, of Netley, and in the antitetanus serum; and I feel sure that many more equally effective and similar means will soon be in our hands against other diseases. But it is not the practical physician and surgeon who will discover them, but the intellectual

devotees of pure science, steadily pursuing, through good and evil report, their self-appointed task of universal beneficence.

More Universities and Research Institutions needed.

I think I have said enough to prove that the progress of pure science means medical progress also, and that it is to the men of pure science that we must still look to hold aloft for us the torch of progress. I can only hope that in the great advances still to be made Great Britain and Ireland may play their worthy part; but I confess I have my misgivings unless some improvements are made in our educational arrangements. As the result of several recent Augusts spent in many of the Continental capitals, I have been struck with the thoroughness and scientific spirit everywhere there manifested; being very different, I regret to add, from the antiscientific spirit which characterises most of the wealthier and more cultivated classes in this country. This antiscientific spirit is only eradicable by the better education of the young. To what other cause can we ascribe the fact that although the aniline dyes were discovered in 1858 by an Englishman, Dr. W. H. Perkin, yet almost the whole of their manufacture is now carried on in Germany. One company, the Badische Anilin und Soda Fabrik, employs 146 scientific chemists, 75 civil engineers, and 6300 workmen in one alone of its three factories. I know not if any words of mine will reach the eyes of those who govern in this country, but if they do I will implore them to give to our youth more facilities, similar to those enjoyed on the Continent and in America. I know that much has been done, and no fault is to be found with our primary education. The National Physical Laboratory and the Jenner Institute of Preventive Medicine, have been established, the former in principle, the latter in reality, thanks to the splendid munificence of Lord Iveagh. New colleges, universities, and technical schools have been started, and some fifteen to seventeen research scholars are appointed every year, one-half of whom, I may remark, proceed at once to Germany to make their researches! What has been done, however, is as nothing to what ought to be done.

The defences of the empire must not be overlooked, but of what avail will be those defences if our trade is gradually lost for want of exacter knowledge? For trade and medical progress, and those very defences themselves, rest entirely upon progressive science.

Depend upon it, there is as much scientific ability and aptitude for research in the country of Cavendish and Priestley, of Faraday and Kelvin, of Darwin and Huxley, of Jenner and Lister, of Rayleigh and Ramsay, as in any foreign nation; but here the chances and opportunities are much fewer, and many a great discoverer that might have been, one of whom it might have been written that—

> He broke his birth's invidious bar,
> And grasped the skirts of happy chance—

spends his untrained energies in futile efforts and vain regrets, and wholly wastes his commanding intellect for the want of instruction and opportunities easily accessible.

But the cost? It would be as nothing in proportion to the gain, and a Faraday, a Koch, or a Pasteur would be a cheap purchase at a million. Whereas France, Germany, and the United States educate at their universities approximately one student in every 1500 of the population, we in

the United Kingdom are content with less than 1 in 2000. Whereas Germany spends £753,000 a year on its twenty-two universities and France £740,000 a year on its sixteen, we in Great Britain and Ireland are content with less than one-fifth of that amount, namely, £135,339 among seven of our thirteen. Although the Leland Stanford University of California has received no less than nine millions sterling from private munificence, and many others of the 480 American universities and colleges have likewise received huge sums—I may specify the Chicago University £200,000, the Johns Hopkins University £1,400,000, and the Cornell University over £1,000,000—no such general largesse is, I fear. to be expected from our British millioniares. The help must come from Government, supplemented by local contributions, or it may not come at all. Yet the matter is one of life or death for the country, for more and more every year the victory in every department, and the predominance in arts and trades, will pass to the possessors of the latest knowledge, the deepest science, and the most perfect and most economical processes. I cannot but believe that this great need will soon be remedied, and that at the cost of a few battleships real universities of teaching and research, like the new University of Birmingham, will be established in every large town, and technical and research institutions—there are twenty-five of these in Germany and forty-three in America—in most of the smaller ones. At present we are feeling keenly the competition of America, a competition that is not likely to diminish, for it possesses over 100 schools of chemistry, and has 10,000 students of engineering. This competition we can only hope to resist by imitating the foresight, generosity, and enthusiasm for scienee of the American Governments and of the American men of wealth. And here I would wish to quote the words of Dr T. K. Pearsons, of Chicago, himself the donor of £800,000 to its University. "Monuments and monumental arches," he says, "once finished become dead memorials; their active function ceases. The school and college live, just as does the memorial in cold statuesque granite, but *their* functions never cease; education goes on from day to day, thinkers are moulded, the nation is served, humanity is benefited in the college."

Oh! that these words may find a practical echo in this country! Examinations are of secondary consequence. China is the most examined country in the world, and look at it. Original work done under the professor's eye and at his suggestion is far better than any examinations, and the true ideal of a real university must always be in the words of Paulsen, "*Die Erkentniss der Dinge durch freie Forschung.*" ("True knowledge from free research.") Let us hope that we shall not have long to wait for many such universities, where the best and most practical scientific education will be open to all at low cost, as in the University of Michigan, where the fees do not exceed £7 a year, or at no cost at all, as in the State of Massachusetts, and in many of the Western States of America. There is no time to lose. The Consultative Committee on Education is, however, a good beginning, which I sincerely hope will soon be followed by the appointment of a Minister for Education, a functionary to be found in nearly every civilised state except our own. It is not trades unions and skilled workmen that can ensure the continued prosperity of a nation, but the trained intelligences that direct its energy; and we may rest assured that whichever nation shall elevate highest the general standard of education will be the nation which will lead the world. Since

the preceding section was written I have to record the magnificent—I hope it will prove infectious—generosity of Mr Andrew Carnegie, with his gift of £2,000,000 to the Scottish Universities, one portion of it to be applied mainly to the teaching and endowment of research, and the rest to replace the fees hitherto paid by their students. Happy Scotland! that not only receives about one-third of the whole parliamentary grant for university education, but has found besides such a princely and far-seeing benefactor as Mr Andrew Carnegie.

NEURECTOMY.[1]

BY FRANK BOOTH, M.R.C.V.S., COLWYN.

MR. CHAIRMAN AND GENTLEMEN—I propose treating the subject I have chosen from a practical point of view, and describing the operation as I have seen it performed and as I have performed it myself, the diseases for which it is performed, the results of the operation, and choice of subject to operate on. I shall first describe the operation.

Neurotomy is now performed on the hind limb as well as the fore, but as I have never performed the operation on the hind limb, or the median operation in the fore limb, nor seen it performed, I shall in this paper confine myself to the plantar operation in the fore limbs.

I have performed plantar neurotomy in three ways, the high, middle, and low operation.

The High operation is performed by cutting the metacarpal nerve immediately above the fetlock joint, in the hollow between the posterior edge of the bone and the anterior edge of the perforans tendon.

The Middle operation is performed on the fetlock joint, just where the same nerve passes over the bone.

The Low operation by cutting the posterior branch of the same nerve, just below and to the back of the fetlock joint.

The subject to be operated on should be prepared by feeding lightly for a day or two previously, and if a gross fat subject he should be physicked. The leg to be operated on should have all the hair clipped close off proposed seat of operations; this should be done the day before the operation, the leg being afterwards well washed with warm water and soap, the water containing Jeyes' fluid, all the soap well rinsed off and the leg bandaged with a linen bandage previously well soaked in a 1·20 solution of carbolic acid. On the morning of the operation remove the bandage, wash the leg again with *cold* water containing Jeyes' fluid, rinse off well and again bandage slackly with same bandage soaked in a 1·20 solution of carbolic acid. If the leg has not been washed the day before the operation, it should be well washed with cold water containing Jeyes' and soap, rinsed and bandaged with a soaked bandage as long as possible before operating.

When ready to operate have your animal cast with ordinary hobbles in a grass field, or on a clean bed of straw; when down keep on his side, have a good man at the head, and you will require two other assistants, one to stand in front and the other behind your animal.

If you are going to perform the high or middle operation, tie a good rope as thick as a halter shank round the leg to be operated on, just above

[1] Read at the Annual Meeting of the North Wales Veterinary Medical Society on September 10th, at Chester.

the knee, and give to the assistant standing behind the horse, then tie a similar rope round the foot of the same leg and give to the assistant standing in the front of the horse, and it is necessary when operating on the inside to have a rope round the upper leg just above the fetlock, and held by the assistant standing behind the horse.

Have all your instruments ready in an enamelled dish containing enough 1·100 solution carbolic acid to cover them: the instruments required are a sharp scalpel, one sharp needle threaded with suturing silk, one blunt needle threaded with thin string, and a pair of ordinary dissecting forceps.

After putting a sack or mat in a convenient place to kneel on, wash your hands well in a carbolic solution, get your assistants to pull their ropes tight, and the man at the head to keep the head down and as still as possible.

To perform the High operation, feel for the artery (by the pulse) in the hollow just above the fetlock joint, and between the posterior edge of the metacarpal bone and the anterior edge of the perforans tendon, cut down over the artery right through the skin from above downwards along the course of the artery, then wipe the wound from above down with your finger, or swab made by wrapping cotton wool in a rag, the whole having been well soaked in a 1·20 solution of carbolic acid, and afterwards dried; never use sponges, tow, or cloth. After wiping the blood out of the wound you will see at the bottom the sheath containing the nerve, artery, and vein, pick the lower edge of this up with the forceps, pass the point of your scalpel through it, the cutting edge pointing upwards, and slit the sheath up from the bottom to the top of the wound *very carefully* or you may also slit a blood-vessel, then wipe wound downwards again with your finger or swab, and you will distinctly see the nerve like a white untwisted string at the back, the artery, as a rule, immediately in front of it, and the vein in front of the artery. After making sure of the nerve, and if you have any doubt it is easy to make sure by squeezing with the forceps, and if it is the nerve you are squeezing the horse jumps, pass your blunt pointed needle down between the blood-vessel and nerve, under the nerve, and take it up in the string, tying it up and leaving about 6 inches of string to get hold by, raise the nerve gently with this string, pass your scalpel under it and cut it off at the top of the wound as high as possible, then pull the cut end downwards as far as possible, and cut off at the lower end of the wound, removing at least an inch of nerve.

If care is taken to *slit* the sheath containing nerve, artery, and vein up, and no dissecting done, afterwards a blunt-pointed needle used to take the nerve up, it (the nerve) will be found quite loose in its sheath and no dissecting will be required before cutting it, but if the sheath has been dissected, or a sharp needle or instrument made for the purpose be used to take the nerve up, then some of the sheath will be included in the string, and you will have to dissect the nerve loose to the top and bottom of the wound before cutting.

After cutting the piece of nerve out, wipe the wound again downwards with your finger or swab, and if any vessels have been cut you will require to stitch the wound up at once to stop the bleeding, but if no bleeding from vessels has taken place, pass three sutures through the lips of the wound and tie a knot on each end of them, leaving the sutures about 4 inches long and untied; when you have finished operating put a linen

bandage that has been well soaked in a carbolic solution round the wounds fairly tight, get your horse up, up in a clean box and rack him up. In a couple of hours after remove the bandage, wash the wound well with a cold solution 1-50 of carbolic acid, being careful to remove every particle of blood, then tie your sutures tight enough to bring and keep the edges of the wound in apposition *only*. Be careful not to draw your sutures too tight. Put over each wound a pad of tow which has been well soaked in a 1-50 solution carbolic acid, and well dusted on the wound side with iodoform, and hold in position with a soaked bandage.

Pack horse up for the first night. Next day readjust the bandage and pads of tow, and turn the horse out to grass, or loose in box; on the 4th day remove bandage and pads of tow, and have wounds exposed, dusted well with iodoform; the sutures may be taken out on the fifth or sixth day. Where a blood-vessel has been cut, leave the stitches long when suturing, and tie with a bow; then the day after open the wound, wash it well with cold solution of carbolic acid 1-50 and re-tie the sutures, after dusting the wound well with iodoform.

To perform the Middle operation feel for the nerve as it passes over the fetlock joint; it is just under the skin and can be easily felt like a cord slipping backwards and forwards by pressing on the skin just over it. Cut down on to it by making an incision from the front backwards and downwards, passing across the nerve, pick up the nerve just as in the high operation, and pull it gently upwards and forwards until exposed at the superior anterior edge of the wound, and as there is not so much of the sheath exposed in the wound by cutting across, it may be necessary to slit it more after picking the nerve up. Cut the nerve through as high as possible, then pull it downwards and backwards and cut off at the bottom edge of the wound, and afterwards treat just as in the higher operation.

In the Lower operation you require to have the leg out of the hobble, and secured by ropes held by assistants and cross hobbles. You feel down the metacarpal nerve over the fetlock joint to the place where it divides into three branches, then follow the posterior branch down to about the middle of the pastern, and cut down on to it along its course just as in the higher operation, and perform in exactly the same way.

The best and most practical of the three operations is the middle one; the nerve is there easier found, there is less tissue around it, and as you cut across the hair, and not in the same way as it grows, there is less blemish afterwards. The lower operation I have found of no use.

Diseases for which the operation is performed.—These are lameness from navicular arthritis, ringbone, and sidebone where the lameness is not amenable to any other treatment.

For navicular arthritis I have operated with success and otherwise. In cases where there is persistent lameness, without much alteration in the structures, the foot fairly wide at the heel, and the frog not very small, the operation may be performed successfully nine times out of ten. But if the foot be long and narrower at the bottom than coronet, and the frog become a mere mark, and both fore feet in the same condition, the operation is rarely successful.

For incurable lameness from ringbone and sidebone, it is particularly effective, the pain ceases and so the lameness disappears, and afterwards the foot grows and expands, becoming wider and thicker, so removing

pressure from the diseased parts, and the actual cause of the lameness, or at least part of it, is so removed.

In cases of developing ringbone, where there is a tardy growth of the ring and great pain, owing to the increased blood supply after neurotomy, the bone grows much quicker, and so the pain which caused the lameness disappears; the mechanical lameness, of course, is always there.

Results.—These are good or bad, there seems to be no medium here. I need not mention the good results further than to say that as a rule, where the operation is a success, the animal goes sound for two to four years, but in some cases much longer. I remember Mr. Faulkner, of Manchester, operating on a horse belonging to Mr. Higginbottom, cab proprietor, of Manchester, and of Llandudno later, when I was a pupil fifteen years ago, and until last summer this horse worked regularly on the Llandudno streets in a landau. I lost sight of him at the beginning of last summer, and cannot say now what has become of him.

The next best result is where your case is neither better nor worse; in this case no harm has been done, even if no good; this is a very rare result, however.

Another result is complete breaking up of the tendons which pass under the navicular joint. This occurs in cases where there has been considerable structural alteration in the foot and navicular joint before the operation. This is rather a common result, as the operation is often put off until the animal cannot possibly be worked any longer. The first symptom of this breakdown is a soft pulpy swelling in the hollow of the heel, which soon extends up the leg; at first the animal goes on its heels a little, then more and more, toe goes up until ultimately no part of the walking surface of the foot comes to the ground at all. There is no treatment of any avail, and the sooner the animal is destroyed the better. A post-mortem of a leg of this sort shows the tendons (flexors of the foot) to have completely lost their attachment, become soft and gelatinous and incapable of any resistance, and the wonder is that the horse could walk at all.

Another bad result is where the navicular bone breaks after the operation. This occurs in cases where there has been old-standing disease of the navicular bone, and immediately after the operation, the pain having gone, the animal uses the part freely, and by extra pressure breaks the bone. In this case the toe goes up and fetlock down suddenly, swelling of the hollow of the heel and leg now follows, and you get a similar leg to the one in breakdown. Of course there is no treatment.

Another bad result which does sometimes occur is for a tumour to grow on the end of the nerve where it has been cut. This result I have never seen myself, but from what I read about it, it is the result of cutting the nerve too short, that is, not pulling it up to the top edge of the wound before cutting, and then cutting as high as possible. If the nerve is not cut off as high as possible it does not retract out of the wound, but the cut end remains in the wound, and may be included in the stitches when the wound is sutured up; in this case the cut end of the nerve becomes irritated by the healing of the wound, the pressure from the stitches or exposure, and you have a tumour form on the end of it; the treatment is to cut it off, being careful this time to cut high enough up.

If care is taken to clean the wound well, remove every particle of blood about two hours after the operation, and the lips of the wound brought

together then when they are discharging a sticky sort of serum, the sutures tied tight enough to keep the edges of the wound in apposition only, the wound will heal by first intention, and will require no care at all. Where the wound does well and the operation has been a success, the animal, if a light horse, is fit for light work in a fortnight after the operation, but heavy horses should have more time, five or six weeks at least, as their work necessitates greater strain on the parts affected, and if put to work too soon may bring about breakdown.

Choice of Subject.—Of course, Mr. Chairman and gentlemen, in practice you choose your subject first, but in this paper I have left it until last, as I think it the least important part of the paper, inasmuch as you rarely even get the chance of choosing a subject. You are requested by a client to get a horse to work as soon as possible that is incurably lame in the foot, and your only means of doing so is to unnerve even though the foot is narrow and the horse interferes badly. But where it is possible to choose a subject it should be a horse in good health, with clean legs, no grease or gumminess about them, no marks of interfering, where there is a good wide foot, wide at the heels, although in my opinion this is the least important of all, for after the operation the foot grows to a surprising extent, and where you had a narrow shelly foot before operating you will soon have a good wide one. Never operate on a horse with pumice feet, and if possible never operate on a horse that plats. Of course the subject chosen should be lame from an incurable disease of the foot. Never operate where there is any chance of curing the lameness by any other means, for although there are hundreds of horses going sound to-day that have been unnerved, which would be unworkably lame if not unnerved, it is an operation which should only be performed as a last resource, as the horse is so much more liable afterwards to other troubles, such as treads, &c.

Now, Mr. Chairman and gentlemen, I have exhausted all the useful knowledge I have on this subject, so I shall conclude, and hope we shall have a good discussion which will lead to further knowledge.

THE BRITISH CONGRESS ON TUBERCULOSIS FOR THE PREVENTION OF CONSUMPTION.

Midland Counties Veterinary Medical Association Delegates' Report.[1]

At a recent meeting of the Midland Counties Veterinary Medical Association, when an invitation was received to send representatives to the British Congress on Tuberculosis, it was readily accepted, and a great honour was conferred upon your President and Honorary Secretary by their appointment as delegates to that important assembly. It now, therefore, becomes their duty, as it is also their pleasure, to present to this Association some account of their stewardship, and to review, as briefly as possible, the great work which the Congress had in hand. It may be said at once that the success of the gathering greatly exceeded the anticipation of the promoters. The Congress would have achieved a noble end if the only result had been the enormous impetus to pathological inquiry caused by the sensational

[1] Submitted at a meeting held on Sept. 11th.

theory propounded by Dr. Koch, but it did more than this. The congregation under one roof of some 2500 British and foreign delegates, among whom were many of the most eminent physicians and pathologists in the world, marked an important epoch in the crusade against consumption, and the direct outcome of the proceedings has been the appointment by His Majesty the King (as recently as last week) of a Commission to inquire into and report upon this vital question.

The Congress sat from July 22nd to July 26th last, the Queen's Hall, London, being engaged for the occasion. Two years ago the then Prince of Wales, as President of the National Association for the Prevention of Consumption, nominated some of the delegates who went to Berlin to take part in the first Congress on consumption, and it was then impressed on the British representatives that a similar meeting in London was greatly to be desired. His Royal Highness at once gave his sanction, and agreed personally to become President of the Congress. The death of the Queen and his ascent to the Throne alone prevented the carrying out of this arrangement, but he nominated a worthy deputy in his illustrious kinsman, the Duke of Cambridge.

The opening ceremony was worthy of the occasion. Supporting His Royal Highness were such distinguished personages as the American aud French Ambassadors, the Ministers of Portugal, Denmark, the Netherlands, and Greece, the Marquis of Lansdowne, Earl Cadogan, Lord Strathcona, the Lord Mayor of London, Lord Lister, Lord Derby, Sir James Blyth, Professor Clifford Allbutt, Sir James Crichton Browne, the Duke of Northumberland, the Marquis of Bath, Earl Spencer, Colonel FitzGeorge, Professor Koch, and the leading representatives of British, Foreign, American, and Colonial States and Associations.

In connection with the opening ceremony a telegram was forwarded to the King, whose reply was received later in the day. Therein His Majesty expressed the hope that the gathering would further assist in meeting the dire disease which has baffled the most distinguished physicians so long.

Upon all the details of the various sections it is not the intention of your delegates to report. Much of the business, though interesting and instructive in a general way, can scarcely be said to affect this Association, as the humble representatives of which your delegates attended. The Housing question, the provision of Sanatoria, Climatology in its application to consumption, and other similar problems affecting more particularly our medical friends, were present in profusion, but it was the veterinary section, so ably presided over by Sir George Brown, C.B., and the consideration of tuberculosis in animals with its attendant danger to the human race that monopolised the attention of your delegates.

First and foremost must be mentioned the address of Dr. Koch, which attracted the attention of the civilised world. It contained the remarkable announcement that tuberculosis in cattle and in the human being was not intercommunicable. The eminent Professor claimed to be fully convinced by his experiments that human tuberculosis could not be transmitted from mankind to cattle, and, what was of much greater consequence, he was led to believe that a similar disease in cattle did not infect human beings. These are Professor Koch's exact words:—"Considering all the facts, I feel justified in maintaining that human tuberculosis differs from bovine, and cannot be transmitted to cattle. It seems to me very desirable, however, that these experiments should be repeated elsewhere, in order that all doubt

as to the correctness of my assertion can be removed. . . . But now, how is it with the susceptibility of man to bovine tuberculosis? This question is far more important to us than the susceptibility of cattle to human tuberculosis, highly important though that is too. It is impossible to give this question a direct answer, because, of course, the experimental investigation of it with human beings is out of the question."

Here, by way of parenthesis, your delegates may mention that one or two intrepid persons who appear to have greater faith in Professor Koch's theory than the majority, have offered themselves as subjects for experiment, but whether their services to mankind in this direction will be utilised is open to question.

Dr. Koch continued as follows :—"If the bacilli of bovine tuberculosis were able to infect human beings, many cases of tuberculosis caused by the consumption of alimenta containing tubercle bacilli could not but occur among the inhabitants of great cities, especially the children. And most medical men believe that this is actually the case. In reality, however, it is not so. That a case of tuberculosis has been caused by alimenta can be assumed with certainty only when the intestines suffer first, *i.e.*, when a so-called primary tuberculosis of the intestine is found. But such cases are extremely rare. Among many cases of tuberculosis examined after death I myself remember having seen primary tuberculosis of the intestine only twice."

The surprise, not to say the incredulity, with which these assertions were received was very manifest in the remarks of subsequent speakers, who voiced a general fear lest Professor Koch's views should lead to a relaxation of the effort to secure purity of meat and milk supplies, which, altogether apart from tuberculosis, is of supreme public importance. Needless to say, as a great London journal was careful at the time to point out, the argument of the great German bacteriologist could not be accepted at once by all who had been working under a precisely opposite supposition.

The opposition was led by a distinguished and honoured member of our own profession, and always a welcome guest at Midland meetings—Professor M'Fadyean. He attacked Dr. Koch's theory intelligently and boldly, and his grasp of the situation raised veterinary science yet higher in the estimation of cultured men. Professor M'Fadyean, whose paper was entitled "Tubercle Bacilli in Cows' Milk as a Possible Source of Tuberculous Disease in Man," first confessed that he was overwhelmed at finding himself in a position which compelled him to offer any criticism on the statements of the greatest living authority on tuberculosis — the world-renowned discoverer of the tubercle bacillus, and the man to whom we were mainly indebted for our knowledge of tuberculosis. Then, with that skilful marshalling of facts so peculiar to the Principal of the Royal Veterinary College, Professor M'Fadyean completely carried his audience with him in an adverse criticism of what Lord Lister described as Dr. Koch's "startling views." "That bovine and human tuberculosis were identical diseases was generally supposed to have been finally determined by Dr. Koch himself," said Professor M'Fadyean, when he discovered that the human and the bovine lesions contained bacilli that were identical in morphological, tinctorial, and cultural characters, and showed that the artificial cultures from both sources produced indistinguishable effects when they were employed to infect a variety of animals. The labours of hundreds of workers during the succeeding eighteen years produced nothing in serious

conflict with the conclusi n that human and bovine tuberculosis were identical diseases, but they brought to light what appeared to be additional evidence of this identity, such as the discovery that tuberculin produced a specific reaction in tuberculous cattle, whether human or bovine bacilli had been employed in its preparation. Opinions varied as to the frequency with which this transmission of tuberculosis from one species to the other occurred, but practically never within the last eighteen years regarding the possibility and probability of such reciprocal infection.

Then Professor M'Fadyean closely examined "the grounds upon which they were asked to discard convictions that appeared to rest on such a solid basis," and in conclusion expressed "the earnest hope that the Congress would not indorse the view that it was inadvisable to take any measures to prevent the transmission of tuberculosis from the lower animals to human beings. To justify the introduction of measures to that end, it was not necessary to contend that this was a common method of infection, or that the danger arising from milk could for a moment be compared with that present in human sputum. "The inhalation of human tubercle bacilli expelled from the bodies of human patients was, doubtless, the great cause of human tuberculosis, and every practicable means of preventing infection in that way ought to be employed; but, at the same time, they ought not to concede to the milkmen the right to sell tubercle bacilli, even if they were assured that—like Dr. Koch's experimental pigs—they had nothing to fear beyond the development of 'little nodules here and there in the lymphatic glands' of our necks and 'a few grey tubercles in our lungs.'"

This final sally, appreciated by those who had read Dr. Koch's paper in detail, provoked hearty laughter and sympathetic cheers, for, as Sir James Crichton Browne subsequently declared, Professor M'Fadyean had rendered fascinating what was, in reality, a grim and unæsthetical subject. The views expressed by Professor M'Fadyean, and shared by so many others present, confirmed the suggestion that further investigation was necessary before Dr. Koch's conclusion could be accepted, and the satisfactory culmination of it all has been, as has already been mentioned, the appointment of a Royal Commission. So much for the important question raised by Dr. Koch, and treated all too summarily in this brief report.

Another very important matter that engaged the attention of the Congress was the control of milk supplies, and here again the veterinary profession was appealed to for assistance and advice.

Professor Delépine, of Owen's College, Manchester, opened the subject with an interesting paper, containing valuable statistics on the subject of the tuberculin test as a means of combating the scourge we all dread.

Another paper on the subject of Tuberculosis and the Milk Supply was read by Mr. J. A. W. Dollar, of London, who, as will be readily understood, emphasised the importance, not to say the necessity, of duly qualified veterinary surgeons being entrusted with the task of examining the source from which the milk supplies of our great cities are derived.

Mr. James Laithwood, Chief Veterinary Inspector for Cheshire, who also read a paper dealing with Tuberculosis and the Milk Supply, contended that the good preventatives of the spread of the disease were (1) the establishment of a State Veterinary Service to exercise functions co-ordinate with those of the Medical Officer of Health; (2) the inclusion of Tuberculosis in the Contagious Diseases (Animals) Act; and, further, the adoption of other equally important measures.

Professor Dewar contributed an important paper on "The Diagnosis of Tuberculosis in Animal Life," and the subsequent discussion was taken part in by many prominent veterinarians, of whom not a few are frequent attenders at our Midland meetings. Professor Dewar confined his observations to the diagnosis of the disease in the bovine species as the one of greatest importance to the veterinary profession. He dwelt at some length upon the value of tuberculin as a diagnostic agent, contending that "notwithstanding the extent to which it has been decried, it is by a long way the most reliable, the most practicable, and the most uniformly useful means of diagnoscing latent tuberculosis that we possess." Your delegates think it almost needless to add that the views of Professor Dewar concerning the value of tuberculin were the views of nearly all present.

Mr. James King, Chief Veterinary Inspector of the City of London, opened what proved to be a most profitable debate on the subject of "Tuberculosis and the Milk Supply." As was only to be expected, a gentleman possessing such facilities for inquiry as Mr. King, furnished the Congress with much valuable information. He presented in attractive form a table giving the results of post-mortems which he himself had made for the detection of tuberculosis. He gave it as his opinion that there was a growing demand for foreign meat and a corresponding decrease in the demand for the flesh of home-bred animals, and salesmen justified the encouragement of this foreign trade by asserting that the carcases of home-fed cattle were too often condemned on account of their being tuberculous. He concluded his most interesting paper with the following statement:— "I am convinced from personal observation that the principal cause for the falling off of our home live-stock markets is tuberculosis, and we are not improving it by our present voluntary efforts to purify some of our stock by the use of tuberculin, as this undoubtedly throws a much larger number of tuberculous animals on the market, and increases the risks of the butcher, and thus making the purchase of home animals more of a gamble than a legitimate trade. Is it then surprising (he added) that he purchases foreign meat to supply his customers instead of the home article."

The concluding day of the Congress in the Veterinary Section was devoted to the discussion of legislative and other measures to combat tuberculosis among animals. This was opened by Professor M'Eachran, Chief Veterinary Inspector of the Canadian Government, who urged the desirability of educating the public themselves by furnishing them with the most important scientific and practical facts known about the disease. Much good, he said, had been done in Canada by the distribution broadcast of pamphlets dealing with the various phases of tuberculosis. This topic was spoken to by most of the leading veterinarians present, who offered a number of valuable suggestions for legislative purposes. Chief among these may be cited the following:—

(1.) Tuberculosis should be included in the list of Contagious Diseases.

(2.) All foreign animals admitted for breeding or dairy purposes should be tested by the tuberculin test, and tuberculous animals prohibited from entering.

(3.) Tuberculin should be controlled, and none but qualified veterinary surgeons allowed to use it; all reacting animals to be reported, marked, and quarantined.

(4.) All animals showing clinical symptoms of tuberculosis, especially disease of the udder, lungs, uterus, or bowels, should be killed at once.

(5.) All testing, other than imported animals, should be by voluntary application for a test of the entire herd, and the expense to be borne by the State.

(6.) Disinfection of premises should be ordered by special regulations, the carrying out of which should be superintended by Government officials.

There was a multitude of other topics bearing upon the detection and treatment of tuberculosis, but time will not permit of your delegates entering into them in detail. It has been their desire, briefly, yet all too imperfectly, to treat of those matters which specially interested them, and which would be most likely to interest this Association. Your delegates put in full time at the Congress, nor did they, as became two discriminating members of an honourable profession, neglect the many pleasant social engagements which their attendance in London entailed upon them. There were receptions and entertainments in abundance. The Lord Mayor of London received the members of the Congress at the Mansion-House, and the Earl of Derby, who was President of the Organising Council, also issued invitations for a reception. Garden parties were given by the Duke of Northumberland and Sir J. Whittaker Ellis; the Royal Botanic Society and Zoological Society threw their grounds open to the Congress free; and many leading clubs and institntions extended to the delegates the benefits of temporary membership. The Central Veterinary Medical Society also entertained the veterinary delegates to an excellent dinner. And from an educational point of view, a visit to the Congress Museum was not to be despised. The arrangement of the very numerous specimens was wonderfully complete, and the catalogue was a store of knowledge in itself. Divided into three sections, the Museum served to illustrate the pathology, the treatment, and the prevention of tuberculosis; and the principal hospitals and colleges, not omitting our own College at Camden Town, contributed of their best.

Your delegates, in conclusion, can only repeat their grateful acknowledgment of the inestimable privilege of being permitted to represent this important Association at what they trust will prove an historic and epoch-marking Congress.

(Signed) JOHN BLAKEWAY, Birmingham, *President.*
HENRY J. DAWES, West Bromwich, *Hon. Sec.*

Mr. M'GILL eulogised the work of the delegates, and said it would be impossible for him to say aught that was not known to every one present as to the extensive knowledge they possessed. The paper just read was a highly interesting one, and the importance of the subject, which had exercised the public mind a good deal of late, could not be over-stated. He moved that the expenses of the delegates be defrayed by the Association.—(Hear, hear.)

Mr. J. MALCOLM seconded, and the motion was carried unanimously.

It was also resolved, on the proposition of Mr. MALCOLM, seconded by Mr. M'GILL, that the delegates' report be printed and circulated amongst the members.

It was decided, on the motion of the HONORARY SECRETARY, seconded by Mr. CARLESS, to hold the next quarterly meeting at Derby.

A hearty vote of thanks was accorded to the President for his services, Mr. M'Gill observing that no one was more fitted to occupy that position than Mr. Blakeway.

BY THE PRESIDENT'S INVITATION.

The business portion of the programme having been disposed of, those present adjourned to another room, where there was in readiness a splendid luncheon, of which they were invited by the President to partake.

At the close the host submitted from the chair the only toast on the programme, "The King." They were now, he said, at Shakespeare's town, a place where their American Cousins took pleasure in visiting, and their thoughts flowed out in sympathy with them in regard to the attempted assassination of the American President, and more particularly to Mrs. M'Kinley and their relations.—(Hear, hear.) He felt that the words of their National Anthem,

> Confound their politics,
> Frustrate their knavish tricks,

were not half strong enough for the twentieth century, and as far as the anarchists were concerned, he for one would be only too pleased to see the torture-chamber and the rack reintroduced.—(Hear, hear.) They, in England, were particularly fortunate in having such a son of such a mother as head of the English nation, a son of the finest Englishwoman. There was but one Shakespeare, and only one Queen Victoria, and it was a pleasure to all to see their King walking so faithfully in her footsteps.—(Applause.)

The toast was duly honoured, after which

Mr. W. S. CARLESS said that, in spite of the order which their worthy President appeared to have laid down, there was one other toast which they ought to drink before separating, and that was the healths of Mr. and Mrs Blakeway.—(Applause.) He had known Mr. Blakeway and the family for years. His father and three brothers were all good sportsmen, and two were leading men in the provinces. As President of the Association, Mr. Blakeway had shown himself to be the right man in the right place, and he felt certain he would enjoy a successful year of office. He only trusted that the members would rally round him in a proper way. He felt that the members did not attend meetings so well as they might. The Association met quarterly, and he thought it hard if the member could not attend four times a year and support their President, especially when they had one like Mr. Blakeway.—(Applause.)

In reply, the PRESIDENT said he had taken upon himself to invite two ex-Mayors of Stratford to the luncheon, Dr. R. Latimer Greene and Mr. W. Hutchings, who had done them the honour to accept their hospitality.—(Applause.) Mrs. Blakeway and he (the speaker) were exceedingly obliged for the reception given to the toast. A good deal of credit should be given to the secretary (Mr. Dawes), who had made the arrangements. They thought when selecting Stratford-on-Avon that it would be an inducement to the members to attend, but unfortunately it clashed with the Rugby and Derby shows, and it was also the St. Leger day.

The health of Mr. H. J. Dawes was also very heartily drunk, and that gentleman briefly acknowledged the compliment.

The party was then photographed, and the remainder of the afternoon was given up to enjoyment.

The various places of historic interest in and about Stratford were duly visited, under the guidance of Mr. W. Hutchings and Mr. F. W Evans, whilst boating on the river was a source of attraction to many.

H. J. DAWES, F.R.C.V.S, *Hon. Sec.*

PATHOLOGICAL ANATOMY OF CORONARY RINGBONE.

BY M. UDRISKI, OF BUCHAREST.

THE author considers that a knowledge of the pathological anatomy of ringbone is necessary in order that a correct opinion may be given as to their origin, viz., whether hereditary or acquired.

M. Udriski's studies on the subject have been (*A*) macroscopical, (*B*) microscopical.

A. At the commencement of their development, ringbones consist of a vascular fibrous tissue that is only partly ossified, and is very porous and painful. Ossification gradually takes place, and the surface becomes uneven and the shape of the growth irregular. The first stage of the process of formation consists of the appearance of red points of a greater or lesser depth of colour in the compact tissue of the bone, at first under the articular cartilage and afterwards under the periosteum, and inversion of the ligaments. The size of these points of colour is variable, but the aspect is always the same. When this process takes place under the articular cartilage, it frequently causes destruction of it; and the pathological process will extend from one phalanx to the other, eventually causing union or true anchylosis. False anchylosis is, however, much more common, and is due to alteration of the tendons or ligaments.

B. Whether the ringbone is articular or peri-articular, changes are always observed in the bony tissue, and rarefied ostitis can be traced through all its stages, to be succeeded by reparation or condensed ostitis. The true processes invade the articular surfaces, and bring about true anchylosis. If it extends under the periosteum, the growth assumes the form of peri-articular ringbone, and their growth takes place very slowly from the centre to the periphery, especially at the points of insertion of the lateral ligaments.

The author gives the following classification of coronary ringbone in the horse:—

A. According to position,	.	Articular ringbone. Peri-articular ringbone.
B. Articular ringbone,	.	Marginal.
According to position,	.	Central. Circular or total.
C. Peri-articular,	.	Partial.
According to position,	.	Complete.

(*Annales de Médecine Vétérinaire*, Sept. 1901.)

JOSHUA A. NUNN,
Vety. Lt.-Colonel.

VERDICT IN THE AIRDRIE QUEY CASE.

(*Reported in our last issue*).

SHERIFF CAMPION on 30th July issued his judgment in the action at the instance of Mr. C. Graham, farmer, Airdrie, Kirkbean, against Mr. Thomas Crawford, cattle dealer, Stranraer:

"Finds (1) that on 20th August 1900 the pursuer purchased at Wallet's auction mart, Castle-Douglas, twenty-four Ayrshire bulling queys, exposed by and belonging to the defender at the price of £192 sterling; (2) that it

was a condition of the sale under which the queys were sold to pursuer that they should be clear of the bull, and that defender warranted them to be so; (3) that at the time of the sale fourteen of said queys were not clear of the bull, and that pursuer has suffered loss through depreciation in the value of said queys: Finds that said queys being at the time of sale disconform to the warranty given by defender, pursuer is entitled to an abatement of the price as damages, assesses said damages at £49 sterling, being at the rate of £3 : 10s. for each of said fourteen queys: Finds pursuer further entitled to the sum of £1 : 10s., being the veterinary surgeon's charges for examining said queys: Decree is therefore given for £50 : 10s. and expenses."

The following abstract of a note by his Lordship accompanies the interlocutor:—"As to the alleged warranty given by defender with these queys, we have the very correct evidence of Mr. Wallet and of Mr. Gibb as to the terms on which the queys were entered and sold. This leaves no doubt as to the warranty given by the defender, under which they were sold, and, in question of Mr. Gibb, 'dearly sold.' This brings us to the question whether the queys were, as averred by pursuer, disconform to such warranty at the time of sale. The queys sold by defender at Castle-Douglas on 20th August appear to have been part of a lot bought by him at Clenoch, Stranraer, the previous Tuesday. Defender admits that he never asked any warranty with them, so it is needless to say that he was not given one. Yet he does not appear to have hesitated to give to pursuer the warranty under which he bought. To pursuer this warranty was of the utmost importance, as these queys were bought for the special purpose of calving in the following October or November, Airdrie not being a dairy farm. After the sale the twenty-four queys were given by pursuer to a drover Slimmon to be driven to Redwell, about five miles from Castle-Douglas, and next day on to Airdrie. Soon after their arrival there pursuer seems to have entertained suspicions that some of them were in calf, and to have delayed sending his cheque to Mr. Wallet till defender passed his word that if anything went wrong he would put it right. Pursuer would appear to have had some reason for such suspicion, for, leaving out of question the episode of the picked calf in January, these queys began to calve on 7th May, the calf born on that date dying. On 8th May arrived the first calf that lived, and it was followed by two on the 13th, one on the 14th, 15th, 18th, and 22nd of May, and one on 1st and 3rd June, while, when Principal Williams visited Airdrie on 6th June, there were other queys in calf which might calve any time. In Principal Williams' opinion the calves in question appeared to be pure Ayrshire, and to have been born at their proper period.

"Now, this proper period is thirty-nine to forty-one weeks—say forty weeks on an average, though it may vary seven or ten days, or even a little longer at certain times and under certain circumstances. We do not here, however, require to consider the evidence as to cases of abnormal gestation, when we have an unbroken series of births which can be amply accounted for in the ordinary course, and find the queys calving steadily just about the rate one would anticipate with queys that had had the bull running regularly among them over a period previous to 20th August. Some of the queys must almost to a certainty have been bulled previous to 20th August, while the latest dates are within a reasonable margin. For instance, the calf born on 8th May, the two born on 13th May, and those born on 14th

and 15th May, had it been subsequent to 20th August, were unlikely according to the authorities, to have all survived, or if they did survive must necessarily have shown signs of immaturity. But they all do survive and exhibit no signs of immaturity.

Clinical Papers.

A CASE OF STRONGYLUS ARMATUS.

BY W. L. GASCOYNE, M.R.C.V.S., ALNWICK.

On July 30th I was asked to look at a three-years-old draught filly, which the owner said had not been thriving at all well for about a month previously. On visiting the case I found the mare in very low condition, staring coat, mucous membranes pallid; temperature 106° F.; pulse 84—almost imperceptible. I first examined the teeth, and found these were quite correct, but noticed that even after this very slight operation the respirations were greatly accelerated, and the heart-beats could be distinctly heard when standing several yards away from her side.

On auscultating the chest, the lung sounds appeared to be normal, but the two heart sounds were almost identical, and could be heard much more plainly on the right side than on the left.

Percussion revealed that the heart was hypertrophied, but the lungs were resonant throughout.

I formed the opinion that the mare was suffering from some acute heart disease, but could not state definitely what it was (none of the sounds usually associated with diseases of the heart could be heard); I also suspected the presence of worms, although I could not find any myself, nor had the attendant observed any.

My treatment consisted in the administration of cod liver oil, small doses of diffusible stimulants, febrifuges, and bitters, and the best of any food the patient cared to take; at the same time I informed the owner that I considered the case hopeless.

On August 4th, my next visit, I found the mare in much the same condition as before, but the temperature was reduced to 104° F., and on auscultating the region of the heart a sound very like a low whistle could be heard on the right side. From this I concluded that the heart trouble was endocarditis, but still I could not in any way satisfy myself as to the cause.

On August 7th no improvement was noticeable, but on examining the fresh fæces I found several worms from half an inch to an inch long, which I recognised as *Strongylus armatus* and *S. tetracanthus* (no doubt these were brought away by the cod liver oil, which I have found to be a very good vermifuge, especially in young animals). This was the explanation of the whole case. I now prescribed thymol 20 grs., to be given in milk, and continued the other treatment.

On my next visit, August 9th, a great number of similar worms were to be found in the fæces, but the animal was rapidly getting weaker, and was ultimately found dead in her box on August 11th.

I made a post mortem examination on August 12th. There was rapid decomposition, probably due to the hot weather prevailing at the time; the heart was flabby and hypertrophied, and endocarditis was well marked on both sides, but more particularly on the right; the lungs were blackened,

and rapidly decomposing; stomach healthy, as also were the small intestines; the small blood-vessels supplying the large intestines were inflamed, enlarged, and in many were small cysts, if such they may be termed, which were found to contain an embryo strongyle; on the anterior mesenteric artery a heautiful aneurism was seen, and on section this was found to consist of a calcareous material embedded in the surrounding tissues, and in the centre were several small embryo strongyles.

The aorta was healthy in all its length.

In the cæcum and colon were enormous quantities of adult worms, all dead, but whether this was due to the thymol or to the rapid decomposition it would be impossible to say.

The liver was greatly eularged, congested, and hard.

The kidneys were flabby and emphysematous.

I am not aware that this disease is at all common in Britain, and trust that it will remain rare.

POLYURIA.

BY W. OWEN WILLIAMS.

I SIMPLY desire to place on record an unusual case of the above disease.

Two hunters being conditioned were taken to Portobello sands in the mornings to do their work, and one morning, Thursday, September 12th, in its course were taken in to the sea. As they splashed the water about they licked their lips and lapped some of the water. The next day their appetites were gone, they became intensely thirsty and urinated frequently and copiously. I saw them the following morning, Saturday, and found the usual and well-marked symptoms of "Diabetes insipidus." The groom told me that they had seemingly melted away during the last two days, and were running into skeletons. I treated them in the usual way, and they are now, September 26th, doing their regular work. I need hardly say that a most careful investigation of the food and water showed that they were not the cause of the disease, and I can only account for its occurrence in the two animals simultaneously being owing to the stomachic irritation from the salt water.

A CASE OF VOMITION IN THE HORSE SUPERVENED BY GANGRENE OF THE LUNG.

BY HENRY TAYLOR, M.R.C.V.S., NEW VETERINARY COLLEGE, EDINBURGH.

ON July 26th, whilst attending a brown van gelding which had been showing colicky symptoms for an hour or two previous to my arrival, I noticed the patient give a spasmodic jerk with his head and neck, and immediately afterwards emit a quantity of food from his nostrils. Succeeding this was rather a curious phenomenon. Standing near the left shoulder of the animal one could quite easily observe boluses of food travel *up* the œsphagus (by the antiperistaltic action of its muscular walls), only to be re-swallowed when they had arrived at the pharynx, the downward course being just as easily distinguishable. The process was continuous, the horse meanwhile standing with his head extended and fixed and his jaws and lips firmly closed, so that he did not allow any more food to be ejected from his nostrils. At this stage the pulse was regular, 36 per minute, and there was not much depression.

XUM

In the course of an hour or two I returned and found that in my absence he had plastered the walls of his box with the vomited material, which diffused a sour odour throughout the stable. I did not see him vomit again, but the process before described still continued, *i.e.*, the regurgitation of the food up the œsophagus followed by its re-swallowing, and it was performed just as frequently as on my previous visit. The patient did not appear in very acute pain, but had a somewhat haggard look. Later in the day I again saw him, and this time the regurgitation of the food was less frequent; the pulse had increased to 65, but the temperature remained normal. An ectasia of the œsophagus was noticed in its lower cervical third.

The treatment pursued had been antispasmodics administered subcutaneously, and antiseptics administered per orem, the latter being with difficulty given. The animal showed no inclination either for food or water.

My prognosis was of course unfavourable, having in view the possibility, nay, probability of ruptured stomach.

Next morning I rather expected to find him dead, but instead of that he was standing quietly and appeared free from pain. On closer examination the following were the symptoms presented: the regurgitation of food now took place only at intervals, the pulse at the submaxillary artery could not be felt (the heart was beating 60 times per minute), the nostrils were soiled by food which had been vomited during the night, the respirations were quickened, and the breath had a fœtid gangrenous odour. The comparative infrequency of the regurgitation was a subject on which I congratulated myself, and I had optimistic opinions till I detected the odour of the breath. It was quite patent what this was due to: some of the food had, without doubt, during his efforts of swallowing, found its way into the trachea and bronchi, with gangrenous pneumonia as a result.

During the next three days the breath became still more fœtid, though the regurgitation of food ceased; the temperature rose to 103, the pulse remained about 60, very weak. He ate little or nothing, but drank some thin gruel occasionally. The treatment consisted of inhalations of medicated steam, and the administration of drachm doses of quinine daily.

On the eighth day when I visited him I found the pulse was very weak, and had risen to 84, temperature 104°, respirations 60. On auscultating the chest, I heard at the lower anterior parts of the thorax, on both sides, splashing sounds, which I took to indicate septic pleurisy: these sounds were synchronous with the respiratory movements. In addition there was a copious foul-smelling diarrhœa.

As the case was now hopeless he was destroyed. *Post-mortem.*—Stomach, not ruptured, about two-thirds full of food (mostly hay), villous portion diffusely inflamed, having several patches about the size of a pea of a deeper red than the surrounding part: in the centre of most of these was an ulcer.

Œsophagus was dilated in its lower cervical third for a distance of 3 inches.

Lungs.—No pleuritic effusion. The antero-inferior parts of both lungs contained consolidated areas of varying size of a deeper colour than normal. On cutting into the largest consolidated areas, they disclosed in their midst vomicæ containing a reddish-brown fœtid liquid. These evidently accounted for the sounds heard during life. In the lung substance also there were

numerous white patches about the size of a marble, of the consistence of soft cheese, and in some places small abscesses. Many of the smaller bronchi also contained a reddish-brown fœtid liquid.

Pericardium and endocardium had ecchymosed spots. The outer surface of the intestines looked normal; they were not opened, neither were the other organs examined.

The precise relationship of ruptured stomach and vomition in the horse has not yet been settled, but the above case adds one more to those already recorded of the act occurring without any rupture.

UNUSUAL NUMBER OF TAPEWORMS IN A DOG.

BY HENRY TAYLOR, M.R.C.V.S., NEW VETERINARY COLLEGE, EDINBURGH.

LAST month we had brought to the College an Airedale terrier in good condition, and to all appearances in very good health, to undergo treatment for worms. This was proceeded with by fasting the dog for forty-eight hours, and administering 35 minims of tenaline, in water, that doze corresponding to his weight. In twenty minutes he evacuated a quantity of fæces, and a tangled mass of tapeworms, which on being teased out and counted, was found to be composed of sixty-six tænia. The worms were on an average about ·75 mm. long, and they all appeared to be of one variety, viz., *Tænia serrata.*

Neumann ("Parasites and Parasitic Diseases of the Domesticated Animals"), says that for *Tænia serrata* the number of individuals which Bertholus and Chauveau found in the dog varied from one up to sixty-four, so we may take the above number as exceptional. A day or two after the dog was given another dose of tenaline, but we did not succeed in augmenting our previous number.

Extracts.

CASES OF COLIC.

A TWELVE-YEAR-OLD full-blooded gelding that had never before shown any signs of colic, was one day so severely attacked by it that after two and a half hours death supervened, with manifestations of suffocation. At the post-mortem great expansion and quite considerable thickening of the wall of the rectum was discovered. The thickening especially affected the muscularis, whose strength was as much as 2 cm. About 4 cm. from the opening of the cæcum one could see a series of sac-like projections of the wall of the intestine, which were arranged in a straight line and at equal intervals over a stretch of 15 cm., to the number of seven; besides this, there was also an eighth diverticulum somewhat sideways under the second projection. The sac-formations were of a size varying from that of a cherry to that of a smallish apple, and were, as was afterwards found on opening up of the intestine, crammed with ingesta, consisting of stalks of hay and straw. It could be seen that all the intestines shared in the projection. Between the series of sac-formations and the mesentery was a longitudinal tear 6 cm. long, through which the contents of the intestine had passed over into the abdominal cavity. Here the strength of the muscularis was only

0·5 cm. The case described differs from the so-called Meckel diverticulum, inasmuch as it was a case of eight projections, while the Meckel diverticulum is a stoppage formation.

(*Zeitschr. für Veterinärkunde.*)

In July 1900 there was subjected to examination by me a mare suffering from stricture of rectum. I was told that she had been covered on 26th June, and immediately afterwards considerable bleeding from vagina ensued, which did not stop till an hour had passed. The veterinary surgeon, who examined the patient on the same day, noted, in addition to slight signs of colic, rapid pulse, lessened appetite, and normal temperature, a torn vagina wound at the level of the *portio vaginalis uteri*. On the following day there appeared small balls of fæces covered with slime; temperature remained several days somewhat too high, while appetite grew better. Gradually the wound became smaller, so that only one's finger could enter it. Thereafter appeared signs of colic, and the mare grew leaner and leaner and voiding of fæces more and more difficult. This was her state when brought to me.

Present state.—Condition middling; from the vagina flowed a mucopurulent secretion; repeatedly defecation consisted, after strenuous efforts, of three small dung-balls; patient groaned while voiding. Examination *per vaginam* gave following results—3 cm. to the left over the *portio vaginalis uteri*, at ten o'clock,[1] was a 3 cm. broad cleft-like wound into which only one joint of the index finger could be inserted. The wound went still deeper, but the canal became narrower. The direction was towards the excoratio recto-vaginalis. After the vagina had been widened and lighted up with Polonsky's speculum, the cleft-like aperture became clearly visible, and a sound could be introduced. By this means it was discovered that the canal was 8 cm. deep, and therefore ran into the excoratio. The walls of the canal were callous. Exploration of the rectum gave the following results:—about 26 cm. distant from the anus the rectum was very much narrowed. The narrowed part measured 5 cm. long from front to back. In front of the narrowed place the rectum was widened, and therein was a heap of fæces. During peristalsis and straining the lumen of the narrowed part was 6-7 cm. The upper wall of the rectum showed nothing abnormal. At the narrowing the under wall of the rectum felt very hard; there was especially on infiltration of the excoratio recto-vaginalis in which the upper wall of the vagina and the under wall of the rectum participated. The whole infiltration formed a firm swelling, with a diameter of 10 cm. Pressure made the animal show signs of pain; matter flowed from the fistular opening into the vagina. Though the extent of this parametral infiltration could not be gauged with certainty, treatment had to be directed towards bringing the latter to resorption in the hope of thereby lessening the stricture of the rectum. And especially in this last complication did danger threaten. If the rectum was not regularly twice daily emptied, or precaution taken for thin defecation, the sick animal showed symptoms of colic.

Treatment.—Massage of the infiltrate with hand introduced into the rectum is a practice which brings such danger with it that it cannot be recommended. I therefore determined to try and do away with the infiltra-

tion by means of electrolysis after the opening of the fistular canal had been sufficiently widened in the vagina.

(1.) Into the fistular canal was introduced a bougie of mercur. præcip. rubr. and cocoa-butter, 2·5 cm. long, and 1·5 cm. broad. This was pushed with the finger into the canal, and as high as possible. On the third day the bougie fell out in small pieces. The aim of this treatment was of course to stimulate the walls of the canal, and thereby facilitate the flowing away of the secretion from the infiltrate.

(2.) The rectum was once daily emptied by hand, and a great quantity of 1 per cent. cooking salt solution poured in.

(3.) From a galvanic battery an electrode was introduced into the rectum at the spot of the infiltrate, and the other electrode *per vaginam* into the fistular canal. The electrodes consisted of twisted copper wire. The current was passed along for quarter of an hour. After a few days the induction current was employed. This treatment was pursued ten days. The results were extremely satisfactory. After eight days the fistular canal was nearly closed and the stricture of the rectum so far reduced that defecation took place without pain, and the accumulation of fæces in front of the narrowing no longer occurred. The infiltrate had lessened much in extent. After fifteen days it had vanished, and the stricture of the rectum quite passed away. Up to the present the horse is hale and hearty.

(M. G. de BRUIN in the "*Berliner Thierärzt. Woch.*")

[1] I borrowed this designation, says de Bruin, from a communication of Dr. Stoyanor in the *Presse Médicale*. Stoyanor says he got the terminology from St. Mark's Hospital, London, and calls it "poétique aussi bien qu' ingénieuse." He would like to see it widely employed—for example, in ophthalmology—to designate the position of an *ulcus corneæ*. The explanation is very simple. In treating diseases of the rectum, an imaginary circle is drawn, with the anus as centre; this is supposed to represent the dial of a clock. For rupture of the cervix or for vaginal wounds, rupture of the vestibule, &c. this description is very useful. It is very curious to hear students speak of an eight o'clock or twelve o'clock fistula.

A CURIOUS CASE OF FOOT-AND-MOUTH DISEASE.

VETERINARY-SURGEON HAUPTMANN reports the examination of the carcase of an ox which was slaughtered by order of the authorities at Salzburg. He found all the lymphatic glands greatly enlarged and much swollen. On the muzzle there was loss of substance in two patches; one was the size of a florin, and round, the other was larger, jagged, and irregular, with upturned caps and brownish-red, scurfy, dried-up base. The tissue at the bottom of the ulcers had turned into a yellowish-grey structureless mass. Mucosa of the nostrils and mouth much reddened all over, but without loss of substance. On the tongue, in place of the papillæ, worn patches from the size of a farthing to that of a crown piece, of round and longish-oval form, and of the same nature as the ulcers on the mouth, only the destruction had gone deeper. The mucosa of the maxillary cavity had turned into an ulcerous surface covered with decaying remains of tissue, the epiglottis had in parts fallen entirely away, and the mucosa of the larynx had in places gone like that of the maxillary cavity. Trachea and lungs were in a state of intense hyperæmia, and showed slight hemorrhage. Œsophagus filled with bright yellow-coloured flaky remains of the mucosa. In the vicinity

of the œsophagic opening into the rumen, and on the pillars of the latter, there were great bare patches, caused by decay, of the same character as in malignant foot-and-mouth disease. Second stomach unchanged, abomasum much reddened. Small intestine seemed changed into a rigid tube by infiltration of its walls, and showed on its exterior at short intervals blue-red spots 1 centimetre broad and 10 to 15 long. There were ulcers on the mucosa of the small intestine, surrounded by a red space, and with firmly adhering scurf. The great intestine showed intense hyperæmia. Liver and kidneys were of light brown colour tending to yellow. On the hind legs a destruction of tissue extending to the bones, of the same character as the ulcers on the tongue. The decayed tissue was dry, almost like india-rubber.

Judging by these lesions Hauptmann thought at first that he had a case of malignant foot-and-mouth disease before him. But against this assumption stood the circumstance that after careful examination of the whole herd of which the dead animal had been a member no trace of the disease could be found in any of them; not only this, but the whole district had been declared free from foot-and-mouth disease for six months. It was accordingly assumed that the unusual lesions had been caused by the bacillus of necrosis. The ox before becoming unwell had been driven about the country for ten days by its owner from market to market, and had got its hind feet seriously injured; probably the bacilli had found in these wounds the conditions necessary for their existence. The transference of the bacilli to the mouth, jaws, and to the respiratory and digestive canals had been effected by licking the infected parts. Hauptmann is of opinion that in this case, contrary to usual experience, the bacillus of necrosis had set up the disease independently, and without the co-operation of other bacilli.

(*Thierärzt. Centralblatt.*)

Review.

VETERINARY MATERIA MEDICA AND THERAPEUTICS. By KENELM WINSLOW, B.A.S., M.D.V., M.D. (Harv.). W. R. Jenkins, New York; Baillière, Tindall, & Cox, London. Pp. 750.

THIS latest work on Veterinary Materia Medica is a handsomely bound and printed book of 750 pages 8vo. and in its compilation the various works of Brunton, Wood, Hare, Edes, Ringer, Bartholomew, White, and Main, in human medicine, and Dun, Friedberger, and Frohner, Ellenberger, Koch, Cagny, and Müller have been consulted and drawn upon.

The classification and arrangement of drugs are modelled on that of Hale White's "Materia Medica," and the result is a condition of which we approve. Both the preparations of the United States and British pharmacopœias are included in the most important drugs, and the doses are given both in the metric system and in units of the apothecaries' weight.

The book is divided into several sections, the preliminary part being devoted to definitions, the action, absorption, and elimination of drugs, the general actions of drugs and their actions on the various parts of the economy, followed by a chapter on pharmacy and prescription writing. Then follow the descriptions, properties, and actions of the several drugs

used in veterinary practice, the penultimate chapters being occupied with a dose table, general therapeutic measures, such as foods and feeding, counter-irritants, disinfectants, antiseptics, and deodorants, venesection, transfusion, then an index of diseases with the remedial measures advocated, and lastly, a copious general index.

The book is not altogether free from faults, both of omission and commission, and in many instances the text gives us the impression that the writer has had a limited practical experience. In the chapter on *Potassium Iodide* no mention is made of that drug being used for parturient apoplexy, and in this chapter occurs the following passage:—"'Roaring or Thick Wind,' due to a thickened condition of the laryngeal mucous membrane, or, more commonly, to pressure of enlarged lymphatic glands upon the recurrent laryngeal nerve, when of recent origin, may be cured by the administration of *potassium iodide*." The writer has evidently a hazy idea of these two affections, and moreover gives us the impression that he considers *Roaring* and *Thick Wind* as identical diseases.

Chinosol does not appear to be mentioned, perhaps it has not found favour with our American cousins, or perhaps they have not heard of it.

Under the heading of "Aconite" we learn that at the outset of fevers it should be given frequently—"℥v. for the horse every fifteen minutes for two hours, and afterwards ℥x. hourly." This line of treatment would *not* appeal to us, the author being evidently unaware of the peculiar idiosyncrasy of some horses toward Aconite. Physostigmine, instead of, as the book says, being valuable in obstruction of the bowels from twist in horses, we should think that for that affection of the bowels its use would be contra-indicated.

In inducing anæsthesia in canine and feline patients the author follows the general consensus of American opinion and prescribes ether. We notice that in the Antidotes to Chloroform no mention is made of either ammonia or prussic acid. Creolin, it is said, "does not endanger animal life from absorption." That is not our experience with the use of the various creolin compounds with our smaller domesticated animals.

Notwithstanding the drawbacks in connection with the book, a few of which we have glanced at, it has its good points, and we can recommend it as an advantageous addition to any veterinary library, but much more a book for the practitioner of experience than for the student at college.

Notes and News.

WOUNDED WAR HORSES.

The Hon. C. S. Rolls presided on 27th September at a meeting in Westminster Palace Hotel of the Church Society for Promotion of Kindness to Animals. Among those present were Miss Baden-Powell, the Honourable Fitzroy Stewart, and the Rev. F. Lawrence, hon. secretary. It was resolved to call the attention of the Secretary for War to the need of reform in the Army Veterinary Department. It was stated that in view of the certainty of a conference of the Great Powers next year, when the terms of the Geneva Convention will be reconsidered, it was necessary to rouse public opinion to an appreciation of the need of international agreement for the protection of those who on battlefields mitigate or terminate the sufferings of wounded animals.

CONTAGIOUS DISEASES ANIMALS ACTS, 1864 AND 1890.

SUMMARY OF RETURNS OF OUTBREAKS for the Four Weeks ending 14th September 1901.

ENGLAND AND WALES.

Swine Fever.	Anthrax.	Glanders (including Farcy).	Sheep-Scab (for month of August).
Outbreaks 152 Slaughtered 596	Outbreaks 26 Attacked 36	Outbreaks 123 Attacked 226	Outbreaks 10
SCOTLAND.			
Outbreaks 6 Slaughtered 166	Outbreaks 12 Attacked 18	None.	Outbreaks 7
IRELAND.			
Outbreaks 27 Slaughtered 457	None.	None.	Outbreaks 18

Communications, Books, Journals, &c., Received.

Agricultural Journal (Cape of Good Hope); Berliner Thierärztliche Wochenschrift; La Clinica Veterinaria (Milan); Deutsche Thierärztliche Wochenschrift; Tannoform in Veterinary Medicine (March); The Sanitary Journal; Scottish Medical and Surgical Journal; Veterinary Record; La Riforma Veterinaria; Le Mois Scientifique (Paris); L'Echo Vétérinaire (Liège); Annales de Médecine Vétérinaire (Brussels); Johns Hopkins Hospital Reports; Journal of Comparative Medicine and Veterinary Archives; Recueil de Médecine Vétérinaire; Gazette Médicale de Paris; Modern Medicine; Johns Hopkins Hospital Bulletin; Il Veterinario di Campagna (Moncalieri); Le Bulletin Vétérinaire (Orleans); Revista de Medicina Veterinaria (Bucarist); La Revista Veterinaria (Buenos Aires); Zeitschrift für Fleisch-und Milchhygiene (Berlin).

XUM